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THE POPULAR Science Review

JUL 25 1881

EDITED BY W. S. DALLAS, F.L.S.

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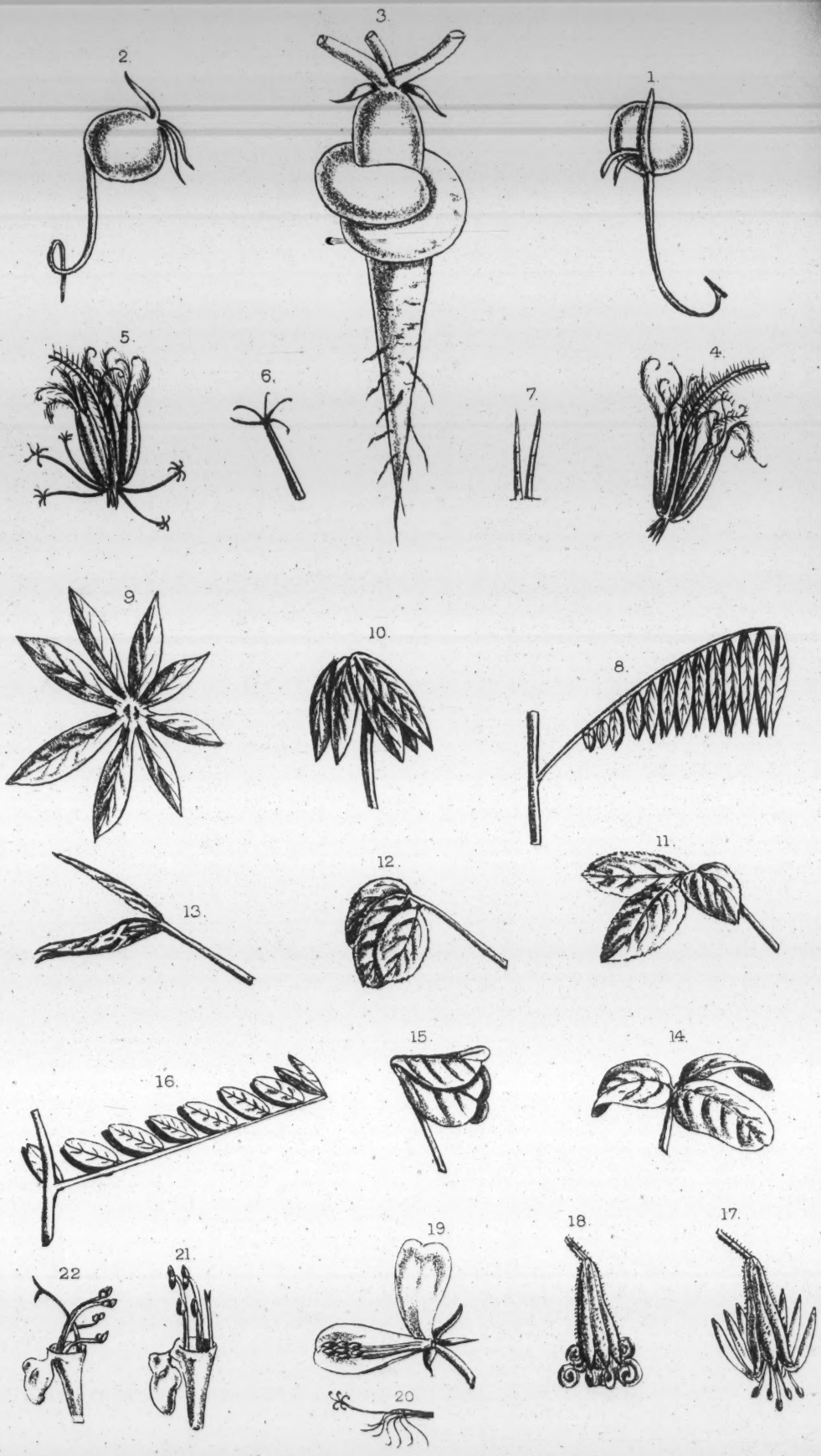

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ILLUSTRATIONS OF PLANT MOVEMENTS.

THE MOVEMENTS OF PLANTS.

By the Rev. GEORGE HENSLOW, M.A., F.L.S., F.G.S.

[Plate V.]

THE old distinction between plants and animals, that the latter can move and the former cannot, has long since been abandoned as unscientific, and only lingers as a copy-book assertion; but that all plants, even when permanently fixed to the soil, have their stems, leaves, flower-stalks, &c., in almost perpetual motion, is a discovery of quite recent times, and notably due to the investigations of Mr. Darwin. His latest work, which bears the title of this article,* is a treatise based on the most careful and elaborate experiments on the motions effected by the different organs of plants; and it was thought that a brief exposition of the chief of these, as well as of some movements peculiar to the parts of flowers not alluded to by Mr. Darwin, might be interesting to the readers of the *Popular Science Review.*

The majority of the movements can be embraced under the single term *circumnutation* and its modifications; it signifies a 'bowing around.' The stem, leaf, or other organ, when circumnutating, bends to all points of the compass successively, with a sort of rolling motion, so that the side which is uppermost in any direction becomes lowermost when it points in the opposite one. The circles or ellipses thus described by the apex of the organ, are most perfectly seen in the circumnutation of the stems of climbing plants; other organs for the most part move in ellipses, but with frequent interpolations of zig-zags, triangular loops, &c. The *projections* of such motions, when observed for some hours, exhibit most complicated and intricate lines; a great number of these projections are given by Mr. Darwin. With regard to the cause of circumnutation, Mr. Darwin says that on the whole we may at present conclude

* *The Power of Movement in Plants.* By Charles Darwin, LL.D., F.R.S., assisted by Francis Darwin. 8vo. London: Murray. 1880.

that increased growth, first on one side and then on another, is a secondary effect; and that the increased turgescence of the cells, together with the extensibility of their walls, is the primary cause of the movement of circumnutation.*

It will be advisable to treat of the different organs in a definite order, so I will proceed to describe the motions of radicles, cotyledons, hypocotyls, and epicotyls of germinating seeds; then will follow those of the stems and leaves of fully developed plants; and finally, the movements effected by the different organs of the floral region.

RADICLES.—The tip of the radicle, as soon as it protrudes from the seed-coat, begins to circumnutate, and the whole growing part continues to do so, probably for as long as growth is maintained; it presumably is aided by this motion in being guided along lines of least resistance. As soon as the radicle has protruded, *geotropism* at once acts upon it. If this force be identical with gravitation, it is gravitation acting in a peculiar way; for although the radicles of germinating seeds, whether in England or at the Antipodes, point to the centre of the earth, yet the influence affects the tip only, for a length of no more than the ·02 to ·03 of an inch, at least, in the cabbage. This minute part, however, at once communicates some influence to a point further back, where the radicle bends downwards in response to it; neither geotropism nor circumnutation can enable the radicle to penetrate the soil. This is effected by the force due to growth both longitudinally and transversely. By means of ingenious contrivances, seeds were grown under resistance; and Mr. Darwin found that transverse growth exerted a force, after six days, of more than 8 lbs., and vertical growth, after twenty-four hours, of at least a $\frac{1}{4}$ lb. The *purchase* for the due effect of these forces is gained partly by the seed being below the soil, aided by the root-hairs, which, in consequence of the cellulose passing into a gum-like substance, fix the radicle to the particles of the soil. The growing part, therefore, acts like a wedge of wood, which whilst slowly driven into a crevice, continually expands at the same time by the absorption of water; and a wedge thus acting, Mr. Darwin observes, will split even a mass of rock.

Another important property of radicles is their extreme sensitiveness to irritants, such as mechanical obstructions, caustic, and injuries by being cut, all of which induce the radicle to *bend away* from the exciting cause, as well as to the presence of moisture, which, on the contrary, induces the radicle to *bend towards* it. Any solid body which gives rise to

* P. 2, 3. I have not here or elsewhere added inverted commas, for throughout this paper I have largely interwoven Mr. Darwin's sentences with my own.

a permanent obstruction to a radicle, causes the latter to deviate from its path till it finds no great resistance; and Mr. Darwin observes that this is the only-known instance of an organ turning *away* from an irritant. By fixing cards to one side of the tips of vertically growing radicles, they were caused to tend upwards, as if trying to escape from the cards, sometimes even forming complete circles, and in one case a radicle actually tied itself into a knot (Pl. V. fig. 3*). The tip in the act of forming a loop, generally rubs against the upper part of the radicle and pushes off the attached card; the loop then contracts or closes, but never disappears. The apex afterwards grows vertically downwards (fig. 2). This sensitiveness is confined to the tip for a length of from 1 mm. to 1·5 mm., while the upper adjoining part of the radicle, for a length of from 6 or 7 to even 12 mm., is excited to bend away from the side which has been irritated. After a time the radicle apparently becomes accustomed to the irritation, as occurs in the case of tendrils, and the irritant no longer affects its downward growth, which is resumed.

After a radicle, which has been deflected by some stone or root from its natural downward course, reaches the edge of the obstacle, geotropism will direct it to grow again straight downwards; but geotropism is a weak force, and the radicle assists it by having its upper part, a little above the apex, sensitive to contact, but acting in a directly opposite manner to that of the tip, for it causes the radicle to bend like a tendril *towards* the touching object, so that as it rubs over the edge of an obstacle it will bend downwards. This downward bending coincides with that due to geotropism, and both will cause the root to resume its original course.

Finally, Mr. Darwin observes,† the several co-ordinated movements by which radicles are enabled to perform their proper functions are admirably perfect. In whatever direction the primary radicle first protrudes from the seed, geotropism guides it perpendicularly downwards; the radicles emitted from the primary are, however, only acted upon by geotropism in such a manner as to cause them to bend obliquely downwards, unless the end of the primary radicle be cut away, then the secondary ones grow vertically downwards. The tertiary radicles are not influenced by geotropism; hence all the rootlets grow in the most advantageous manner, and the whole soil is thus closely searched.

Hypocotyls and Epicotyls.—The radicle having penetrated

* The figure of the radish is not from Mr. Darwin's work, but from a drawing made many years ago, and which would seem now to have received its explanation.

† P. 196.

the soil, the hypocotyl, as the axis below the cotyledons is called, begins to develope, at least in those seeds which elevate their cotyledons above ground; but it is the plumule or epicotyl which alone does so of seeds which retain their cotyledons below the soil. In either case the organ commences its growth in the form of an arch, such being the result of an innate tendency in the case of the plumule to assume that form; the inner concave surface then growing more rapidly than the outer and convex, tends to raise the summit of the arch out of the ground. The apex of the stem, being ultimately freed from the soil, soon straightens itself and becomes erect.

In whatever position the seeds may lie, *apogeotropism* acts upon the arch, and the two legs become vertical; the entire arch circumnutates more or less the whole time as the nature of the soil permits. Mr. Darwin aptly compares the whole process of extrication of the arch from beneath the soil to a man thrown down on his hands and knees, at the same time to one side, by a load of hay falling upon him. He would first endeavour to get his arched back upright, wriggling at the same time in all directions to free himself a little from the surrounding pressure; and this may represent the combined effects of apogeotropism and circumnutation. The man, still wriggling, would then raise his arched back as high as he could; as soon as the man felt himself at all free, he would raise the upper part of his body, whilst still on his knees and still wriggling. This will represent the bowing backwards of the basal leg of the arch, which aids in the withdrawal of the cotyledons from the buried and ruptured seed-coats.

Cotyledons.—Colytedons are in constant movement, chiefly in a vertical plane, and commonly rise up once and descend once in 24 hours; some move much oftener, thus, those of *Ipomœa cœrulea* moved thirteen times either upwards or downwards in 16^h 18^m. As the motion when perfected gave ellipses, cotyledons may be said to circumnutate. In a large majority of instances observed by Mr. Darwin, the cotyledons sank a little downwards in the forenoon, and rose a little in the afternoon or evening, thus exhibiting a certain periodicity in their movements, no doubt in connexion with the daily alternations of light and darkness. When the cotyledons rise or fall to such a degree as to be vertical, or at an angle of at least 60° above or below the horizon, they are said to be asleep; the object gained being, as in leaves, to escape injury by radiation at night.

Stems.—Several experiments with stems of plants of various orders, showed that they continually circumnutate; and in the case of stem-climbers the circumnutation is of the most perfect

kind.* An interesting modification of the process occurs in stolons or runners, which consist of much elongated flexible branches, that run along the surface of the ground and form roots at a distance from the parent plant; the circumnutation is so great in amplitude, that it may almost be compared with that of climbing plants. The stolons are thus aided in passing over obstacles, and in winding between the stems of surrounding plants.

PEDUNCLES.—Flower stems form no exception to axial structures in the habit of circumnutating; but the effect is curiously modified by geotropism in the case of *Trifolium subterraneum*, and by apheliotropism in that of *Cyclamen Persicum*, in both of which plants the object gained is the burying the unripe pods beneath the soil, leaves, &c. The flower-heads of *Trifolium subterraneum* produce but three or four perfect flowers at the base, all the other flowers above consisting only of cylindrical calyx-tubes with stiff spreading lobes, forming claw-like projections (Pl. V. figs. 4–6). As soon as the perfect flowers wither, they bend back upon the peduncles. This movement is due to *epinasty*, a word coined to imply that the upper surface of an organ grows more quickly than the lower surface, and thus causes it to bend downwards. Whilst the perfect flowers are thus bending, the whole peduncle curves downwards and increases much in length, even from 6 to 9 inches if necessary, until the flower-head reaches the ground. At this period the younger, imperfect, central flowers are still pressed closely together, and form a rather rigid conical projection. The depth to which the flower-heads can penetrate varies from ·25 to ·06 inch. In the case of a plant kept in the house, a head partly buried itself in sand in 6 hours; with plants growing out-of-doors, Mr. Darwin believes that they bury themselves in a much shorter time. After the heads are buried, the central aborted flowers increase considerably in length and rigidity, and become bleached; they gradually curve, one after the other, upwards or towards the peduncle (fig. 5). In thus moving, the long claws on their summits carry with them some earth; hence a flower-head which has been buried for a sufficient time forms a rather large ball, the aborted flowers having caught up the earth with the claw-like sepal-lobes, act somewhat like the hands of a mole, which force the earth backwards and the body forwards.

The calyxes of all the flowers are clothed with simple and multicellular hairs (fig. 7), which, on absorbing carbonate of ammonia presented to them, exhibited protoplasmic aggrega-

* For full description of twiners and other climbing plants, the reader is referred to Mr. Darwin's work on *The Movements and Habits of Climbing Plants*. See also *Pop. Sci. Rev.* vol. v. p. 55.

tion. As Mr. Darwin observes that only a few of the flower-heads, which from their position are not able to reach the ground and bury themselves, yield seeds, whereas the buried ones never failed to produce as many seeds as there had been perfect flowers, it may be reasonably conjectured that the object gained is to nourish the ripening seeds directly through their surfaces, and so to supplement root action. The capsules of Cyclamen and of the Wood Sorrel, *Oxalis acetosella*, are occasionally buried, but then only beneath dead leaves or moss.

LEAVES.—The movements of certain leaves which are said to sleep, have long been observed, but it would seem that probably all leaves and cotyledons circumnutate, and that the so-called sleep is only a remarkable modification or development of this general kind of movement, accompanied, however, by other and often complicated motions. The seat of the movement generally lies in the petiole, but sometimes both in the petiole and blade, or in the blade alone. The movement is chiefly in a vertical plane; but as the ascending and descending lines never coincide, there is always some lateral movement, and thus irregular ellipses are formed. There is a periodicity in the movements of leaves, for they often or generally rise a little in the evening and early part of the night, and sink again on the following morning, this periodicity being determined by daily alternations of light and darkness, as already mentioned in the case of cotyledons. These periodic movements occur where there is no *pulvinus*, for where this is found, the movement is amplified into *nyctitropic*, or sleep-movements.

Leaves, Mr. Darwin says, when they go to sleep, move either upwards or downwards; or in the case of the leaflets of compound leaves, forwards, that is, towards the apex of the leaf, or backwards, that is, towards its base; or again, they may rotate on their own axis without moving either upwards or downwards; but in almost every case the plane of the blade is so placed as to stand nearly or quite vertically at night. Moreover, the upper surface of each leaf, and more especially of each leaflet, is often brought into close contact with that of the opposite one, as the upper surfaces appear to require more protection than the lower.

The nyctitropic movements of leaves and cotyledons are effected in two ways, firstly, by means of pulvini, which become ultimately more turgescent on opposite sides; and secondly, by increased growth along one side of the petiole or mid-rib, and then on the opposite side. This difference between the two means of movement consists chiefly in the turgescence of the cells of a fully-developed pulvinus not being followed by

growth. The movements effected by growth on the alternate sides are confined to young growing leaves, whilst those effected by means of a pulvinus last for a long time.

The evil effects which result if sleeping leaflets be prevented from pressing their upper surfaces together, so as to protect them from radiation, were well seen in experiments of Mr. Darwin's, in which he pressed down the leaflets of *Oxalis*, *Marsilia*, &c., so that they could not bring their upper surfaces into contact; the result was that the leaves were killed. Thus of twenty-four leaves of *Marsilia* extended horizontally, exposed to the zenith and to unobstructed radiation, twenty were killed and one injured, whilst a relatively very small proportion of the leaves, which had been allowed to go to sleep with their leaflets vertically dependent, were killed or injured. Mr. Darwin noticed that the difference in the amount of dew on the pinned open leaflets and on those which had gone to sleep, was generally conspicuous, the latter being sometimes absolutely dry, whilst the leaflets which had been horizontal were coated with large beads of dew. Another fact observable was that when leaves were kept motionless, they are more liable to injury than when they were slightly waved about by the wind, and thus got a little warmed by the surrounding air.

Cotyledons, as well as leaves, may sleep; the former seem to do so more commonly than leaves. Of 153 genera observed by Mr. Darwin, one or more species of twenty-six of these genera placed their cotyledons at night so as to stand vertically, having generally moved through an angle of at least 60°. In a large majority of genera the movement is a rising one. In all the species of *Oxalis* observed by Mr. Darwin, the cotyledons are provided with a pulvinus; and he adds that this organ has become more or less rudimentary in *O. corniculata*, in which the amount of upward movement of the cotyledons at night is very variable, but never enough to be called sleep. Similarly, in the Leguminosæ, all the cotyledons which sleep have pulvini. As this organ has been referred to several times, it will be as well to describe it. It constitutes a cushion or joint, and consists of a mass of small cells, usually of a pale colour in the case of that attached to cotyledons, from the absence of chlorophyll, and having a convex outline. The development of a pulvinus follows from the growth of the cells over a small defined space of the petiole being almost arrested at an early stage. As a pulvinus is formed by this arrestment of the growth of its cells, movements dependent on their action may be long continued without any increase in length of the part thus provided; and such long-continued movements seem to be one chief end gained by the development of a pulvinus.

It will be desirable now to give a few illustrations of nyctitropic movements from Mr. Darwin's observations.

Averrhoa bilimbi (Pl. V. fig. 8).—The leaflets of this plant move spontaneously in a very marked manner during the day, are sensitive to touch, and sleep at night, when the leaflets hang vertically down and motionless.

Lupinus.—The digitate leaves of this genus sleep in three different ways. One of the simplest is that all the leaflets become steeply inclined downwards at night, having been horizontal during the day, as those of *L. pilosus* (Pl. V. figs. 9, 10), when asleep are often inclined at an angle of 50° beneath the horizon. In *L. Hartwegii* and *L. luteus*, the leaflets, instead of moving downwards, rise at night, forming a hollow cone with moderately steep sides.

With several other species, the position is remarkable. On the same leaf the shorter leaflets, which generally face the centre of the plant, sink at night, whilst the longer ones on the opposite side rise, the intermediate and lateral ones merely twisting on their own axis; the result is that all the leaflets on the same leaf stand at night more or less inclined, or even quite erect, forming a vertical star.

Melilotus.—The species in this genus sleep in a curious fashion. The three leaflets of each leaf twist through an angle of 90°, so that their blades stand vertically at night, with an edge presented to the zenith; the two lateral leaflets always twist so that their upper surfaces are directed towards the terminal leaflet. This latter leaflet moves in another and more remarkable manner, for whilst its blade is twisting and becoming vertical, the whole leaflet bends to one side, and invariably to the side towards which its upper surface is directed; so that if this surface faces (say) the west, the whole leaflet bends to the west, until it comes into contact with the upper and vertical surface of the western lateral leaflet. Thus the upper surface of the terminal and of one of the two lateral leaflets is well protected. It may be added that the petioles and subpetioles continually circumnutate during the whole twenty-four hours. (Pl. V. figs. 11, 12, 13.)

Trifolium repens.—During the day, the leaflets of this plant are expanded horizontally (Pl. V. fig. 14), but at night the two lateral leaflets twist and approach each other, until their upper surfaces come into contact; at the same time they bend downwards in a plane at right angles to that of their former position, until their mid-ribs form an angle of about 45° with the upper part of the petiole, this change of position requiring a considerable amount of torsion in the pulvinus. The terminal leaflet merely rises up without any twisting, and

bends over until it rests on and forms a roof over the edges of the now vertical lateral leaflets (fig. 15).

Coronilla rosea.—This plant affords an instance of leaflets rising at night from having been horizontal during the day. The leaflets at the same time bend backwards towards the base of the petiole, until their mid-ribs form with it angles of from 40° to 50° in a vertical plane (Pl. V. fig. 16).

Cassia corymbosa, during the day; and the same, asleep, at night.

Cassia.—The nyctitropic movements of the leaves in many species of Cassia are highly complex. Mr. Darwin's observations were made chiefly on *C. floribunda* and *corymbosa*, and he furnishes the following description. The horizontally extended leaflets sink down vertically at night, but not simply, as in so many other genera, for each leaflet rotates on its own axis, so

that its lower surface faces outwards. The upper surfaces of the opposite leaflets are thus brought into contact with one another beneath the petiole, and are well protected, as shown in the woodcut.* The rotation and other movements are effected by means of a well-developed pulvinus at the base of each leaflet, as could be plainly seen when a straight, narrow black line had been painted along it during the day. The two terminal leaflets in the daytime include rather less than a right angle, but their divergence increases greatly whilst they sink downwards and rotate, so that they stand laterally at night, as may be seen in the figure; moreover, they move somewhat backwards, so as to point towards the base of the petiole. In one instance, Mr. Darwin found that the mid-rib of a terminal leaflet formed at night an angle of 36°, with a line dropped perpendicularly from the end of the petiole. The second pair of leaflets likewise moves a little backwards, but less than the terminal pair; and the third pair moves vertically downwards, or even a little forwards. Thus all the leaflets in those species which bear only three or four pairs, tend to form a single packet, with their upper surfaces in contact and their lower surfaces turned outwards. Lastly, the main petiole rises at night, but with leaves of different ages to very different degrees; thus some rose through an angle of only 12°, and others as much as 41°.

The influence of light upon the movements of plants is various. Thus the so-called heliotropic movements are determined by the direction of the light, whilst periodic movements are affected by changes in its intensity. On the other hand, apheliotropism implies that a plant bends from the light, a rare phenomenon, at least in a well-marked degree. Parts of plants under the influence of diaheliotropism place themselves more or less transversely to the direction whence the light proceeds, and are thus fully illuminated. Lastly, some leaves rise or sink or twist so as to avoid great intensity of light. Such a phenomenon, Mr. Darwin suggests, should be called paraheliotropic. All these movements consist of modified circumnutation. Space forbids a detailed description of these effects; but one curious result of Mr. Darwin's investigation is worth recording, and that is, the transmitted effects of light. When, for example, the cotyledon of *Phalaris canariensis* is exposed to light, the upper part bends first, and afterwards the bending gradually extends to the base, and even a little below the ground. By protecting the whole upper half of the cotyledon from light, the lower part, though fully exposed to light, was prevented from becoming curved. Hence it is to be

* I am indebted to the courtesy of Mr. Darwin for the loan of the blocks for these figures.

concluded that when seedlings are freely exposed to a lateral light, some influence is transmitted from the upper to the lower part, causing the latter to bend.

Flowers.—There are many instances of the various parts of flowers moving under the influence of stimuli; but the immediate causes in the different cases have not been so thoroughly investigated as by Mr. Darwin in the case of the vegetative organs, and at present, therefore, but little can be said beyond the fact that they do move. Thus, commencing with bracts, it may easily be seen how the erect bracts of the involucre of the dandelion become reflexed as soon as the fruit is ripened, thus allowing the parachute-like achenes to escape easily. Moving corollas are very numerous. A large series of plants might be mentioned of which the corollas close up, either as soon as the sun is obscured, as *Mesembryanthemum*, *Anagallis arvensis*, *Convolvulus*, &c., or else at evening, such as many Compositæ, including the Daisy and Dandelion, re-expanding on the return of light. Conversely, some night-flowering plants unfurl their petals only at night, coiling them up by day. As an illustration *Silene nutans* may be taken, concerning which Dr. Kerner tells us* that a flower lasts three days and three nights; with the approach of dusk the bifid limbs of the petals spread out with a flat surface, and fall back upon the calyx. In this position they remain through the night; curling themselves up into an incurved spire and becoming longitudinally creased at the same time, on the return of sunlight and a warm temperature. No sooner does evening return, than the wrinkles disappear, the petals become smooth, uncurl themselves, and falling back against the calyx, the corolla is again expanded (Pl. V. figs. 17, 18).

In the Pea family, or Leguminosæ, there are several instances of the corolla having a power to move when irritated. Thus in the genera *Genista* and *Indigo*, the claws of the petals act like springs kept in a state of tension; for when the corolla is touched, as by an insect in search of honey, the claws suddenly curl downwards, and the petals consequently drop vertically, while the stamens, previously concealed within the keel petals, are violently thrown upwards, showering the bee with pollen.†

The movement of stamens is perhaps more curious, and apparently intimately connected with the phenomenon of insect fertilization. As an example of slow movement, *Parnassia palustris* may be mentioned. In this flower, each stamen in succession rises up, places the anther on the stigma, and having shed its pollen, retires and falls back upon the petals. Each

* *Flowers and their Unbidden Guests*, p. 132.

† *Journ. of Linn. Soc.* vol. ix. p. 355, and vol. x. p. 468.

stamen occupies about twenty-four hours in rising up and discharging its pollen, and takes about the same time to recede, the whole period being eight days, but varying according to circumstances of temperature, moisture, &c.*

Berberis furnishes an instance of rapid motion; for if the stamens be touched at the base of the filament, they instantly spring forward and strike the stigma, having previously lain on the surface of the spreading petals. The effects of the irritation on the filament of *Berberis* has been observed and described by M. E. Heckel.† It appears that the cells of the irritable part are arranged in a parallel manner (the back of the filament being insensible). Their contents are yellow and disseminated throughout the cavity. After irritation, they undergo aggregation and contract into the centre of the cell, and the cell-wall is striated transversely. The cells of the back of the filament are contracted in repose, but extended in irritation.

The stamens of the common Lucerne, *Medicago sativa*, as also of other species of that genus, suddenly curve upwards and remain rigidly fixed in an arched condition, having been previously horizontal.‡

Mr. Darwin has described numerous instances in the family Orchidaceæ, in which the *pollinia*, as of the common *Orchis mascula* and others, or of *Catasetum*, &c., have remarkable powers of movement; in the former cases slow, but in the latter, rapid. In every case there is discernible some special adaptation to the fertilization of the flowers by insect aid.§

Stylidium affords another illustration of rapid motion. In this flower the stamens and style are consolidated into a column, which is curiously bent and hangs over one side of the flower. If it be touched near the base, it instantly flies over to the other side. A very similar motion occurs in the pistil of *Maranta*.

Some flowers have the stamens in a certain position on first expanding, but they take up another position subsequently. This I found to be the case with *Alisma Plantago*. On first expanding, the stamens spread out, their anthers being extrorse, but afterwards they curl backwards over the stigmas, thereby in all probability effecting self-fertilization. In several flowers of different species, the filaments retire after the anthers have shed the pollen or fallen off, as in the lemon-scented and oak-leafed *Pelargoniums*, and in *Teucrium Scorodonia*, or wood-sage. In both of these, the anthers mature some time

* Baxter's *British Flora*, vol. i. (70).
† *Bull. Soc. Bot. Fr.*, 1874, vol. xxi. p. 208.
‡ *Journ. of Linn. Soc.* vol. ix. p. 327.
§ See *Fertilization of Orchids*.

before the stigmas, and assume a position adapted to insects to transport their pollen. Subsequently the filaments bend away, and the styles now take up the same position that the filaments had previously held (Pl. V. figs. 19–22).

Space will not allow further descriptions; but enough has now been said to show how extensive and varied are the movements effected by the different organs of plants, and the advantages accruing to them by possessing such powers of motion.

EXPLANATION OF PLATE V.

Figs. 1, 2, 8–16, *after Darwin.*
Figs. 4–7, 19–22, *ad nat.*
Figs. 17, 18, *after Kerner.*

FIGS. 1 and 2. *Zea mays;* radicles excited to bend away from little squares of card attached to one side of their tips. In fig. 2, the card has been rubbed off.

FIG. 3. A radish, of which the radicle had tied itself into a knot, probably in the same manner as in the case described and figured by Mr. Darwin, by the apex continually moving away from some obstruction, until it had passed through the loop, as seen in fig. 2. Reduced one-half.

FIG. 4. Pendulous head of florets of *Trifolium subterraneum;* the fertile flowers reflexed, the abortive still erect, forming a vertical cone.

FIG. 5. The same, with abortive florets, now developed and becoming reflexed.

FIG. 6. A barren floret, enlarged. It consists of the rigid 'calyx-tube,' supporting claw-like sepal lobes.

FIG. 7. Multicellular hairs from the calyx, which exhibit aggregation under the process of absorption of nitrogenous matters. Figs. 4–7 enlarged.

FIG. 8. Leaf of *Averrhoa bilimbi,* asleep, with its leaflets pendulous; much reduced.

FIG. 9. Leaf of *Lupinus pilosus* seen vertically from above, by day.

FIG. 10. Leaf of the same seen laterally, at night. Figs. 9 and 10 reduced.

FIG. 11. Leaf of *Melilotus officinalis,* during daytime.

FIG. 12. Leaf of the same, asleep.

FIG. 13. Leaf of same, asleep, seen vertically from above. Figs. 11–13 enlarged.

FIG. 14. Leaf of *Trifolium repens,* during the day.

FIG. 15. Leaf of same, asleep, at night.

FIG. 16. Leaf of *Coronilla rosea,* asleep, with its leaflets thrown upwards.

FIG. 17. Flower of *Silene nutans,* by night, the petals unrolled.

FIG. 18. The same, by day, the petals being rolled up.

FIG. 19. Flower of oak-leafed *Pelargonium*, to show the position of the stamens *before* fertilization. The style and stigmas immature.

FIG. 20. The same, with filaments reflexed *after* fertilization; the anthers have fallen. The stigmas are now in the position of the anthers.

FIG. 21. Flower of *Teucrium Scorodonia*, to show the position of the stamens *before* fertilization.

FIG. 22. Same, after fertilization, the stamens having retired, while the stigmas now occupy their position.

THE CENTENARY OF THE DISCOVERY OF URANUS.

By W. F. DENNING, F.R.A.S.

THE year 1781 was signalized by an astronomical discovery of great importance, and one which marked the epoch as memorable in the annals of science. A musician at Bath, William Herschel by name, who had been constructing some excellent telescopes, and making a systematic survey of the heavens, observed an object on the night of March 13 of that year, which ultimately proved to be a large planet revolving in an orbit exterior to that of *Saturn*. The discovery was as unique as it was significant. Only five planets, in addition to the Earth, had hitherto been known; they were observed by the ancients, and by each succeeding generation; but now a new light burst upon men. The genius of Herschel had singled out from the host of stars which his telescope revealed, an object, the true character of which had evaded human perception for thousands of years!

The centenary of this remarkable advance in knowledge naturally recalls to mind the circumstances of the discovery, and makes us inquisitive to know what new facts have been gleaned of Herschel's planet now that a hundred years have passed away and we are enabled to look back and review the vast amount of labour which has been accomplished in this wide and attractive field of astronomical research. We may learn what new features have been discerned of the new body, and what additional discoveries in connexion with other planets, unknown in Herschel's day, have been effected by aid of the powerful telescopes which have been devoted to the work. We do not, however, intend dealing with the general question of planetary discovery, for at a glance we are impressed with its magnitude. While a century ago five planets only were known, we now have some 230 of these bodies, and the stream of discovery flows on without abatement through each succeeding year.

The detection of *Uranus* seems, indeed, to have been the prelude to many similar discoveries, and to have offered the incentive to greater diligence and energy on the part of observers in various parts of the world.

Many great discoveries have resulted from accident; and the leading facts attending that of *Uranus* prove that in a large measure the result was brought about in a similar way. Herschel, as he unwearyingly swept the heavens night after night, was in quest of sidereal wonders—such as double stars and nebulæ, and he happened to alight upon the new planet in a purely chance way. He had no expectation of finding such a remarkable object, and indeed, when he had found it, wholly mistook its character. There could be no doubt that it was a body wholly dissimilar to the fixed stars, and it was equally certain that it could not be a nebula. It had a perceptible disc, for when it had first come under the critical eye of its discoverer he had noticed immediately that its appearance differed widely from the multitude of objects which crossed the field of his telescope. He had been accustomed to see hosts of stars pass in review, and their aspect was in one respect similar, namely, they were invariably presented as points of light incapable of being sensibly magnified, even with the highest powers. True there was a great variety of apparent brightness in these objects, and a singular diversity of configuration, but there was no exception to the invariable feature referred to. The point of light was constant, and no striking exception was anticipated until one night—March 13, 1781—Herschel being intently engaged in the examination of some small stars in the region of *Gemini*, brought an object under the range of his telescope, which his eye at once selected as one of anomalous character. Applying a higher power, he noticed that it exhibited a planetary disc, but his instrument failed to define it with sufficient distinctness, and hence he became doubtful as to its real nature. The object was found to be in motion, and subsequent observations led him to the assumption that it must be a comet of rather exceptional type. This appeared to be the best explanation of the strange body, for history contained many records of curious comets, some of which were observed as nearly circular patches of nebulous light, and probably of similar aspect to the object then visible; and apart from this, it must be remembered that the idea of a large planet exterior to *Saturn* was a fact of such momentous import, that Herschel, with a due regard to that modesty which accompanies true genius, refrained from attaching such an interpretation to his observations. He was content to direct the notice of astronomers to it as a phenomenon requiring close attention, and suggested that it might be a comet in conse-

quence of its motion and the faint, and somewhat ill-defined character of its appearance.

From the earliest ages five planets only were known, and

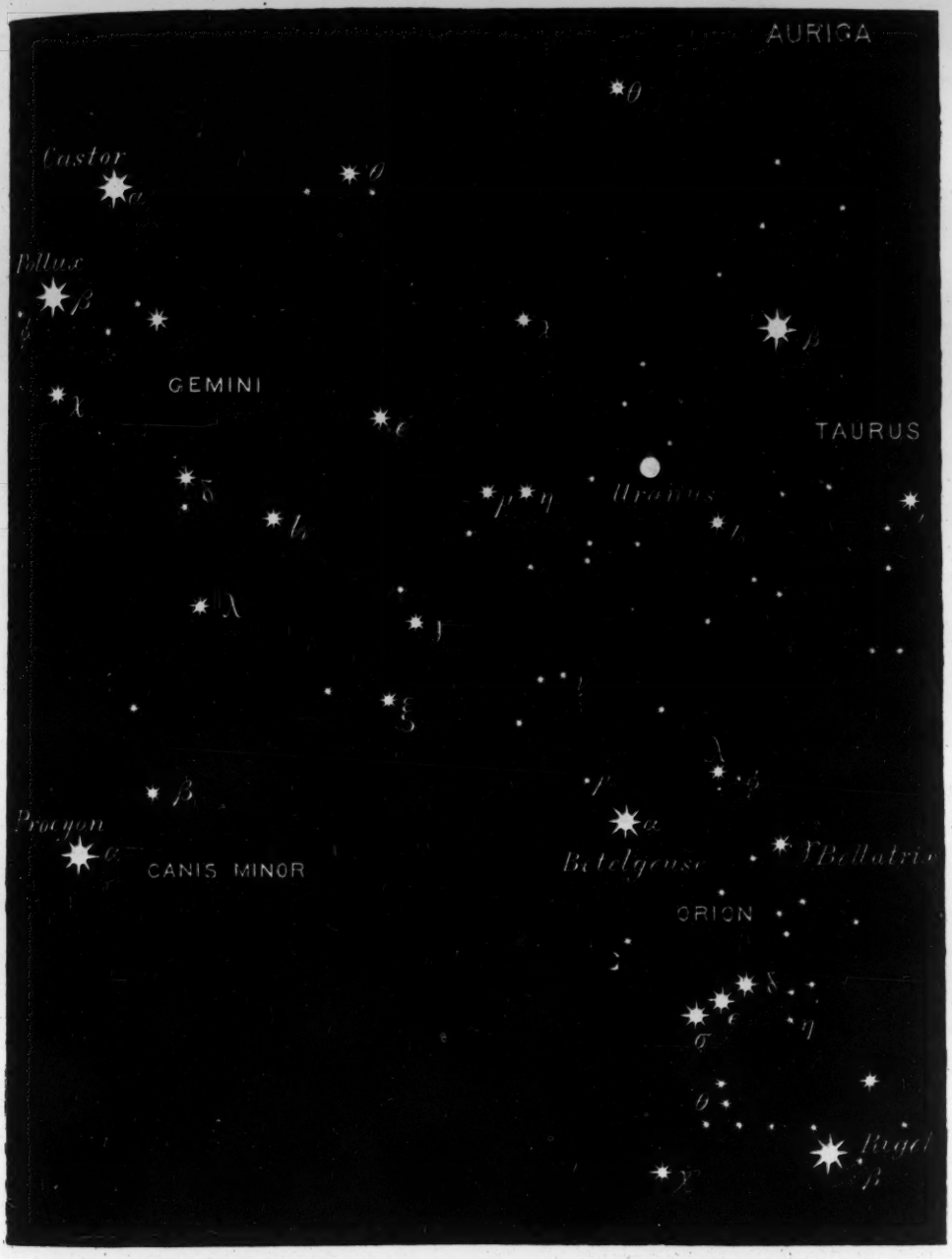

FIG. 1. Approximate place of *Uranus* amongst the stars at its discovery on March 13, 1781.

the discovery of another large planet beyond the sphere of *Saturn* must at once revolutionize existing ideas as to the range of the solar system, and immediately take rank as a scientific event of equal interest to the discovery of the moons of *Jupiter*

or the rings of *Saturn*, which each in their day impressed men with new ideas of the celestial mechanism. But the truth could not long be delayed. The new body being watched, and its orbit rigorously computed from a series of observed positions, revealed its true character, and Herschel was awarded the honour due to the author of a discovery of such importance. His diligence and pertinacity alone had enabled him to search out from amongst the multitude of stars thickly strewn over the firmament, this unknown and well-nigh invisible planet, which during all the preceding years of the world's history had eluded human perception. Men had been all unconscious of its existence, as it had been slowly completing its circuits around the Sun, obedient to the same laws as the other planets of the solar system, and awaiting the hour when the unfailing eye of Herschel should introduce it as the faint and far-off planet girding our system within its expansive folds.

As soon as the existence of the new orb was confirmed, and the fact rendered indisputable, the question naturally arose whether it had ever been seen in former years by the authors of star-catalogues, who could hardly have overlooked an object like this, though its planetary nature had manifestly escaped detection. It was just perceptible to the naked eye, shining like a star of the sixth magnitude, and ought to have been distinguished by those who had reviewed the heavens with the purpose of determining and mapping the positions of the stars. Reference was therefore made to the chief catalogues, when it was found at once that the planet had been unquestionably observed by Tobias Mayer, Le Monnier, Bradley, and Flamsteed. It was several times noted by these observers: by Le Monnier no less than twelve times, and by Flamsteed on six occasions; and it is remarkable that in every instance its true character escaped detection. Neither its special appearance nor its motion attracted attention, so that it was merely catalogued as an ordinary fixed star. Thus Herschel was not anticipated in his discovery. It remained for him, in 1781, to note its exceptional aspect, and to specify it as an object requiring critical investigation. But the early observations above alluded to served a useful purpose in testing the accuracy of the computed orbit, for without waiting many years to compare the theoretical and observed positions, astronomers had, in these old records, a reliable series of points, through which the previous course of the planet could be traced.

The calculations showed that its mean distance from the sun was some 1,750,000,000 miles, and that a revolution was completed in about eighty-four years. It was also found to be a very large planet, greatly exceeding either *Mercury*, *Venus*, the *Earth*, or *Mars*, though considerably inferior to either *Jupiter* or *Saturn*.

Here, then, was a discovery of the utmost importance, and one of the most salient additions to our knowledge which the telescope had ever achieved. The new planet was now definitely assigned its proper place in the solar system, and was regarded as of equal significance with the old planets. True, the new planet of Herschel could not be compared as regards its visible aspect with the other previously known members of our system, but it was nevertheless an object of equal weight. Its vast distance alone rendered it faint. It formed one of the constituent parts of the solar system, which, though separated by immense intervals of space, are yet coherent by the far-reaching effects of gravitation. There is, indeed, a bond of harmony between the series of planetary orbits, which exhibit a marked degree of regularity in their successive distances from the Sun; and though they are not connected by any visible links, they are firmly held together by unseen influences, and their motions are subject to certain laws which have been revealed by centuries of observation.

The question of suitably naming the new planet soon came to the fore. Herschel himself proposed to designate it the *Georgium Sidus*, in honour of his patron, George III., just as Galileo had called the satellites of *Jupiter* the *Medicean stars*, after Cosmo de Medici. But La Place proposed that the planet should be named after its discoverer; and thus it was frequently referred to as *Herschel*, and sometimes as 'the Herschelian planet.' Astronomers on the Continent objected to this system of personal nomenclature, and argued that the new body should receive an appellative in accordance with those adopted for the old planets, which had been selected from the heathen mythology. Several names were suggested as suitable (on the basis of this principle), and ultimately the one advanced by Bode received the most favour, and the planet thereafter was called *Uranus*.

The varying positions of the new body as observed on successive nights, were determined by comparisons with a group of six small stars, termed by Herschel $\alpha, \beta, \gamma, \delta, \varepsilon, \zeta$, and afterwards formed into a constellation under the designation of '*Britannia*,' though it does not appear that this little asterism is acknowledged as one of our constellations. Its position is about midway between *Taurus* and *Gemini*, and the following are the principal stars computed for 1881·0, as given by Mr. Marth:—

Star.	Magnitude.	Right ascension.	Declination.
		h m s	
α	9·0	5 42 6·06	23° 35′ 6″·7 N
η	8·7	5 43 17·82	23 26 7·2 „
θ	8·8	5 44 0·99	23 53 30·8 „
ϵ	8·8	5 45 40·68	23 34 46·8 „

The stars are therefore merely telescopic, and are confined to a small area of space, so that the propriety of adopting the group as a distinct constellation is very questionable. Their positions, close to *Uranus* at the time of its discovery, and the fact that the planet's motion was detected by means of comparisons with them, has given to these stars an historical interest which in future years must often attract the student to their re-observation. But it would be unwise, as forming a bad precedent, to accept a group of stars of this inferior type as meriting to rank amongst the old constellations, when we have numbers of richer groups, situated on their confines, which first deserve such a distinction. However special or unique the circumstances connected with certain telescopic stars may be, and however necessary it may appear to signalize them by a specific title, we are inclined to question the adoption of such means as likely to exercise a wrong influence, inasmuch as it may hereafter originate further innovations of a similar character, and ultimate complications will be certain to arise.

Soon after the discovery of *Uranus*, it was suspected that the planet was encircled, like *Saturn*, by a luminous ring, but on subsequent observation this was not confirmed; and no such appendage has ever been revealed in the more perfected instruments of our own times. Indeed, if *Uranus* displays a peculiarity of constitution in any way analogous to the ring-system of *Saturn*, it must be of the most minute character so as to have thus evaded telescopic scrutiny during a hundred years.

The discovery soon attracted the notice of royalty, and the reigning sovereign, George III., anxious to practically express his appreciation of the valuable labours of Herschel, awarded him a pension of 200*l.* a-year and furnished him with a residence at Slough, near Windsor, and the means to erect a gigantic telescope with which he might be enabled to continue his important researches. This instrument consisted of a reflector on the 'Front-view' construction, with a speculum 4 feet in diameter and of 40 feet focal length. Upon its completion, Herschel immediately began to observe the region of the new planet with the idea of discovering any satellites which might belong to it, for analogy suggested that it was surrounded by a numerous retinue of such bodies. He was soon successful, for, on the night of January 11, 1787, he saw two minute objects near the planet, which renewed observations revealed to be satellites; and he detected two additional ones in 1790, and two others in 1794, making six in all. But the observations were of extreme difficulty. The path of the planet frequently passed near minute stars, and it became hard to distinguish between them and the suspected satellites. Herschel, however, considered he had obtained conclusive evidence of the existence

of six satellites with sidereal periods ranging from $5^d\,21^h\,25^m$ to $107^d\,16^h\,39^m$, and his means of observation being much superior to those possessed by any of his contemporaries it was impossible to have corroborative testimony.

The matter was thus allowed to rest until the middle of the present century, when Lassell, in the pure sky at Malta, endeavoured to re-observe the satellites with a 2-foot reflector. This instrument was considered superior to Herschel's telescope; and the atmosphere at this station being decidedly more suitable for such delicate observations than in England, it was removed there for the express purpose of dealing successfully with objects of extreme difficulty. The results were very important. Mr. Lassell became convinced that *Uranus* had only

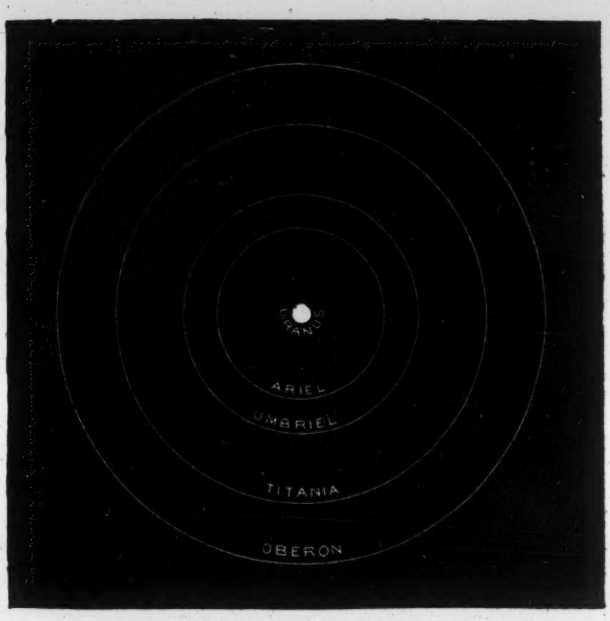

FIG. 2. Orbits of the Uranian satellites.

four satellites, and that if any others existed they remained to be discovered. Two of these were found to be identical with those seen by Herschel in 1787, and now called *Titania* and *Oberon*. The other two, *Ariel* and *Umbriel*, could not be identified with any of those alleged to have been previously detected by Herschel, so that the inference was that they were new bodies, and that the priority of discovery was due to Mr. Lassell; whence it also followed that the older observations were erroneous, and that in fact Herschel had been entirely mistaken with regard to the four satellites he believed he had detected subsequently to 1787.

In November, 1873, a fine 26-inch object-glass, by Alvan Clark, was mounted at the U. S. Naval Observatory at Wash-

ington, and it was soon employed upon the difficult task of solving the problem as to the exact periods of the Uranian satellites. This was very satisfactorily effected, and with distinct and conclusive favour to Mr. Lassell, whose observations were fully corroborated. Only four satellites could be distinguished by the American observers, and their periods, as computed from a valuable series of measures, agreed with those previously derived at Malta. In Appendix I. to the *Washington Observations* for 1873, Prof. Newcomb gave a valuable summary of results,—the first obtained, be it noted, with that splendid instrument which soon afterwards, in 1877, revealed the satellites of *Mars*,—which included the elements of the satellites of *Uranus* as follows :—

Satellite.	Mean Longitude Epoch 1871, Dec. 31, W.M.T.	Radius of Orbit.	Period of Revolution in days.
I. Ariel	21°·83	13″·78	2·52038
II. Umbriel	136·52	19·20	4·14418
III. Titania	229·93	31·48	7·70590
IV. Oberon	154·83	42·10	13·46327

Speaking of the comparative brightness of the satellites, Prof. Newcomb says :—'The greater proximity of the inner satellites to the planet, makes it difficult to compare them photometrically with the outer ones, as actual feebleness of light cannot be distinguished from difficulty of seeing arising from the proximity of the planet. However, that *Umbriel* is intrinsically fainter than *Titania* is evinced by the fact that although the least distance of the latter is somewhat less than the greatest distance of the former, there is never any difficulty in seeing it in that position. From their relative aspects in these respective positions, I judge *Umbriel* to be about half as bright as *Titania*. *Ariel* must be brighter than *Umbriel* because I have never seen the latter unless it was farther from the planet than the former at its maximum distance. . . . . I think I may say with considerable certainty that there is no satellite within 2′ of the planet and outside of *Oberon*, having one-third the brilliancy of the latter; and therefore that none of Sir William Herschel's supposed outer satellites can have any real existence. The distances of the four known satellites increase in so regular a way that it can hardly be supposed that any others exist between them. Of what may be inside of *Ariel* it is impossible to speak with certainty, since in the state of atmosphere which prevails during our winter, all the satellites named disappear at 10″ from the planet.'

Prof. Newcomb mentions that no systematic search for new satellites was undertaken because it must have interfered with the fullness and accuracy of the micrometer measures of the

old satellites, which constituted the main purpose of the observations. Some faint objects were occasionally glimpsed near the planet, and their relative places determined, but they were never found to accompany *Uranus*. The fact, therefore, that no additional satellites were discovered is not to be regarded as a strong point in favour of the theory of their non-existence, because the great power and excellence of the telescope was expressly directed to the attainment of other ends; and moreover the season in which the planet came to opposition was distinctly unfavourable for the prosecution of a rigorous search for new satellites. There can, however, be no doubt that the analogies of the planetary systems interior to *Uranus* plainly suggest that this planet is attended by several satellites which the power of our greatest telescopes has hitherto failed to reveal; and that it is in this direction, and that of *Neptune*, we may anticipate further discoveries in future years when the conditions are more auspicious and the work is entered upon with special energy, aided by instruments of even greater capacity than those which have already so far conduced to our knowledge of the heavenly bodies.

Notwithstanding the extreme difficulty with which the Uranian satellites are observed, the two brighter ones, *Titania* and *Oberon*, discovered by William Herschel in 1787, have been occasionally detected in telescopes of moderate power, and identified by means of an ephemeris which has shown that the computed positions approximately agree with those observed. During the last few years Mr. Marth has published ephemerides of the satellites of both *Saturn* and *Uranus*, and many amateurs have to acknowledge the valuable aid rendered by these tables, which supply a ready means of identifying the satellites, and thus act as an incentive to observers who are induced to pursue such work for the sake of the interesting comparisons to be made afterwards. In one exceptional instance the two outer satellites of *Uranus* appear to have been glimpsed with an object-glass of only 4·3 inches aperture, and the facts are given in detail in the *Monthly Notices of the R. A. S.*, April 1876, pp. 294–6. The observations were made in January, February, and March 1876, by Mr. J. W. Ward of Belfast; and the positions of the satellites, as he estimated them on several nights, are compared with those computed, the two sets presenting tolerably good agreement. Indeed the corroborations are such as to almost wholly negative any scepticism, though such extraordinary feats should always be received with caution. In this particular case the chances of being misled are manifold; even Herschel himself fell into error in taking minute stars to be satellites and actually calculating their periods; so that when we remember the difficulties of the

question our doubts are not altogether dispelled. Extreme acuteness of vision will, in individual instances, lead to success of abnormal character, and certainly in Mr. Ward's case the remarkable accordances in the observed and calculated positions appear to be conclusive evidence that he was not mistaken.

It will be readily inferred that the great distance, and consequent feebleness, of *Uranus* must render any markings upon the disc of the planet beyond the reach of our best telescopes; and indeed this appears to have been a matter of common experience. Though the surface has been often scanned for traces of spots, we seldom find mention that any have been distinguished. Consequently the period of rotation has yet to be determined. It is true that an approximate value was assigned by Mr. T. H. Buffham from observations with a 9-inch reflector in 1870 and 1872, but the materials on which the computation was based were slender and necessarily somewhat uncertain, so that his period of about twelve hours stands greatly in need of confirmation. The bright spots and zones seen on the disk in the years mentioned appear to have entirely eluded other observers, though they are probably phenomena of permanent character and within reach of instruments of moderate size. Mr. Buffham * thus describes them:—

'1870, Jan. 25, 11^h to 12^h in clear and tolerably steady air; power 132 showed that the disk was not uniform. With powers 202 and 320, two round bright spots were perceived, not quite crossing the centre but a little nearer to the eastern side of the planet, the position angle of a line passing through their centres being about 20° and 200,—ellipticity of *Uranus* seemed obvious, the major axis lying parallel to the line of the spots.

'Jan. 27, 10^h to $10\frac{1}{2}^h$; some fog, and definition not good, but the appearance of the spots was almost exactly the same as on the 25th.'

On March 19 glimpses were obtained of a light streak and two spots. On April 1, 4, 6, and 8, a luminous zone was seen on the disk, and in February and March, 1872, when observations were resumed, certain regions were noted brighter than others, and underwent changes indicating the rotation of the planet in a similar direction to that derived from the results obtained in 1870. Mr. Buffham points out that if this is admitted, then the plane of the planet's equator is not coincident with the plane of the orbits of the satellites. Nor need we be surprised at this departure from the general rule, where such an anomalous inclination exists. In singular confirmation of this is Mr. Lassell's observation of 1862, Jan. 29, where he says, "I received an impression which I am unable to render certain of an equatorial dark belt, and of an ellipticity of form." '

* *Monthly Notices R. A. S.*, January, 1873.

Some observations made in 1872–3 with the great six-foot reflector of Lord Rosse, may here be briefly referred to. A number of measures, both of position and distance, of *Oberon* and *Titania*, were made,* and a few of *Umbriel* and *Ariel*, but 'the shortness of the time available (40 minutes) each night for the observation of the planet with the six-foot instrument, the atmospheric disturbance, so often a source of annoyance in using so large an aperture, and other unfavourable circumstances, tended to affect the value of the observations, and to make the two inner satellites rarely within detection.' On Feb. 10, 1872, Lord Rosse notes that all four satellites were seen on the same side of the planet. On Jan. 16, 1873, when definition was good, no traces of any markings were seen. Diameter of *Uranus* = 5″·29. Power 414 was usually employed, though at times the inner satellites could be more satisfactorily seen with 625.

It may be mentioned, as an interesting point, that some fifty years after the first discovery of *Uranus* by Herschel, it was accidentally re-discovered by his son, Sir John Herschel, who recognized it by its disk, and had no idea as to the identity of the object, until an ephemeris was referred to. Sir John mentions the fact as follows, in a letter to Admiral Smyth, written in 1830, August 8: 'I have just completed two 20-feet reflectors, and have got some interesting observations of the satellites of *Uranus*. The first sweep I made with my new mirror I *re-discovered* this planet by its *disk*, having blundered upon it by the merest accident for 19 *Capricorni*.'

In commenting upon the centenary of an important scientific discovery we are naturally attracted to inquire what progress has been made in the same field during the comparatively short interval of one hundred years which has elapsed since it occurred. We have called it a short interval because it cannot be considered otherwise from an astronomical or geological point of view, though, as far as human life is concerned, it can only be regarded as a very lengthy period, including several generations within its limits. Since Herschel in 1781 discovered *Uranus*, Astronomy has progressed with great rapidity, so that it would be impossible to enumerate in a brief memoir the many additional discoveries which have resulted from assiduous observation. A century ago only five planets were known (excluding the Earth), now we are acquainted with about 230 of these bodies; and one of these, found in 1846, is a large planet, whose orbit lies exterior to that of *Uranus*. In fact, the state of astronomical knowledge a century ago has undergone wonderful changes. It has been rendered far more complete and comprehensive by the diligence of its adherents and by the unweary-

* *Monthly Notices R. A. S.*, March, 1875.

ing energy with which both in theory and practice it has been pursued. A zone of small planets has been discovered between *Mars* and *Jupiter* just where the analogies of the planetary distances indicated the probable existence of a large planet. The far-off *Neptune* was revealed in 1846 by a process of analytical reasoning, as unique as it was triumphant, and which proved how well the theory of planetary perturbations was understood. The planet was discovered by calculation; its position in the heavens assigned, and the telescope was then employed merely as the instrument of its detection. The number of satellites which a century ago numbered only ten has now reached twenty; and the discovery in 1877 of two moons accompanying *Mars* shows that the work is being continued with marked success. In other departments we also find similar evidence of increasing knowledge. The periodicity of the Sun-spots, the existence of systems of binary stars, meteor showers and their affinity with cometary orbits, may be mentioned as among the more important, while a host of new comets, chiefly telescopic, have been detected. Large numbers of nebulæ and double stars have been catalogued, and we have evidence every year of the activity with which these several branches are being followed up. In fine, it matters little to what particular department of astronomical investigation we look for traces of advancement during the past hundred years, for it is evident throughout them all, and sufficiently proves that the interest formerly taken in the science has not only been well sustained, but has become more general and popular, and is extending its attractive features to all classes of the community. In Herschel's day, large telescopes were rare. A man devoting himself to the study of the heavenly bodies as a means of intellectual recreation, was considered a phenomenon and, indeed, that appellation might be fittingly applied to the few isolated individuals who really occupied themselves in such work. How different is the case now that the pleasant ways of science have called so many to her side, and so far perfected her means of research as to make them accessible to all who care to see and investigate for themselves the unique and wonderful truths so easily within reach? Large telescopes have become common enough, and there is no lack of hands and eyes to utilize them, nor of understanding, ever ready to appreciate, in sincerity and humbleness, those objects which display in an eminent degree the all-wise conceptions of a great Creator! It is, therefore, a most gratifying sign to notice this rapid development of Astronomy, and to see year by year the increasing number of its advocates, and the record of many new facts gleaned by vigorous observation.

The character of recent discoveries distinctly intimates that in future years some departments of the science will become

very complicated, owing to the necessity of dealing with a large number of minute bodies; for the tendency of modern researches has been to reveal objects which by their faintness had hitherto eluded detection. And when we consider that these bodies are rapidly increasing year by year, the idea is obviously suggested that, inasmuch as their numbers are comparatively illimitable, and there is likely to be no immediate abatement in the enthusiasm of observers, difficulties will arise in identifying them apart and forming them into catalogues with their orbital elements attached, so that the individual members may be redetected at any time. In this connexion we allude particularly to minor planets, to telescopic comets, and to meteoric streams, which severally form a very numerous group of bodies of which the known members are accumulating to a great extent. As complications arise, some remedies must be applied to their solution; and one probable effect will be that astronomers will be induced each one to have a speciality or branch to which his energies are mainly directed. The science will become so wide in its application and so intricate in its details, that it will become more than ever necessary for observers to select or single out definite lines of investigation and pursue them closely; for success is far more likely to attend such exertions than those which are not devoted to any special end but employed rather in a general survey of phenomena. We have already before us some excellent instances in which individual energies have been aptly utilized in the prosecution of original work in some specific branch of astronomy, and we are strongly disposed to recommend such exclusive labours to those who have the means and the desire to achieve something useful. Observers who find one subject monotonous and then take up another for the sake of variation, are not likely to get far advanced in either. In the case of amateurs who use a telescope merely for amusement, and indiscriminately apply it to nearly every conspicuous object in the firmament without any particular purpose other than to satisfy their curiosity, the matter is somewhat different, and our remarks are not applicable to them. We refer more pointedly to those who have a regard for the interests of the science and whose enthusiasm enables them to work habitually and with some pertinacity.

History tells us that the Great Alexander wept when he found he had no other worlds to conquer, and we fear that some astronomers will lament that they have little prospect of discovering anything fresh in a sphere to which our giant telescopes have been so often directed, but this is founded on a palpable misconception. Certain objects, such as comets for example, do not require great power, and the revelation of new meteor-showers is entirely a question for the naked eye. In

fact, it may be confidently asserted that observations undertaken with energy and persistency will, if rightly directed, more than compensate for defects of instrumental power. It is true, however, that in certain quarters we must look to large instruments alone for new discoveries. It would be useless searching for an ultra-Neptunian planet, or for additional satellites to *Uranus* or *Neptune*, or for the materials to determine the rotation periods of these planets, with a small telescope. Every observer will find objects suited to the capacity of his instrument, and he may not only employ it usefully, but possibly make a discovery of nearly equal import with that which rendered the name of Herschel famous a century ago.

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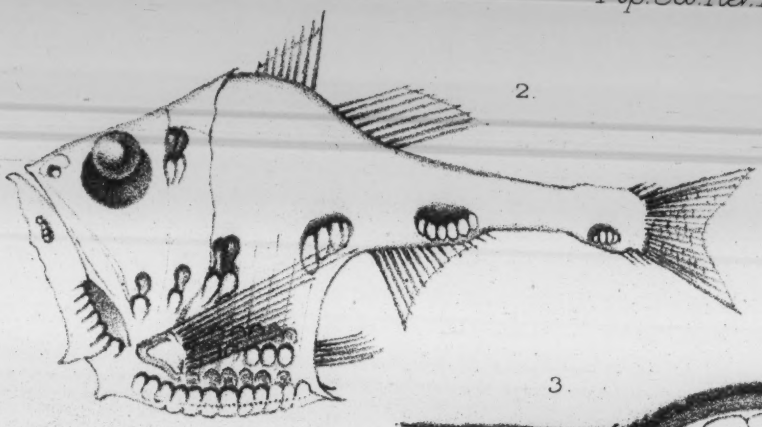

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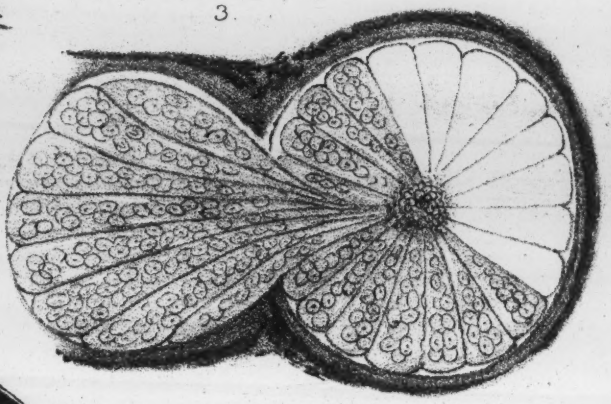

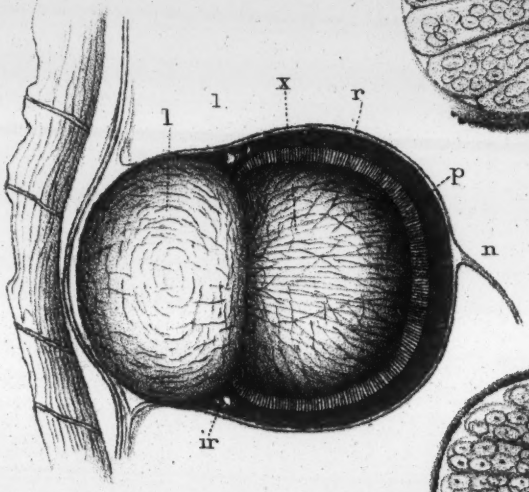

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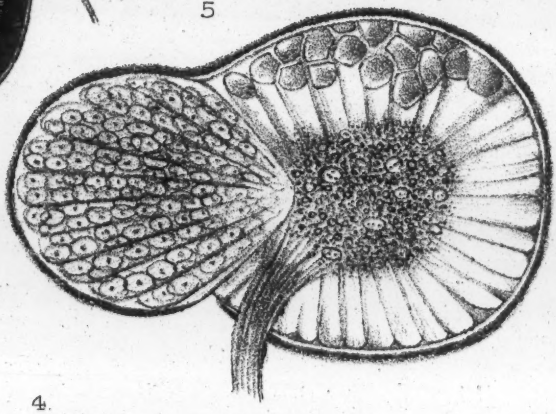

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C. Berjeau, lith.

Mintern Bros. imp.

EYE-LIKE ORGANS OF FISHES.

THE EYE-LIKE SPOTS IN FISHES.

By Professor F. JEFFREY BELL, M.A.

[Plate VI.]

WE are so naturally inclined to take ourselves as the norm or standard in animal organization, that we are almost too easily astounded by any difference in the arrangement of parts or organs which are found in ourselves. With none, perhaps, is this more the case than with the eye; we are so accustomed to associate a pair of eyes with every living creature, that blindness on the one hand, or a number of eyes on the other, excites more astonishment than the reduction or multiplication of any other part. A child in spectacles is the object of a not always respectful pity; and the poet to heighten the horror of his Cyclops makes him one-eyed.

'Ingens, quod torvâ solum sub fronte latebat;'*

And Milton has put much the same feeling into words when he makes Samson say,—

'Why was this sight
To such a tender ball as the eye confined,
So obvious and so easy to be quenched;
And not, as feeling, through all parts diffused,
That she might look at will through every pore?'

A survey, however, of the more important divisions of the animal kingdom, need not be very elaborate to convince us that the arrangement of parts which obtains in the case of our own eyes, and, in broad outline, in that of all the other vertebrated animals, is by no means similar to what we find in the Octopus, the Crayfish, or the marine Worm; yet all these three represen-

* 'With sharpened point that eyeball pierce,
Which 'neath his brow glared lone and fierce.'
Connington.

tatives of these different types have eyes of very considerable complexity and of very different ancestry; and all exhibit obedience to the same physical laws, and present similar optical conditions.

Simpler than these are the eyes of the Star-fishes, and still simpler those of the Leech and of the lowlier Worms, some of which have, as it seems, nothing more than mere pigmented spots, the nervous supply of which has still to be worked out.

But the diversity does not end here: the Leech has ten 'eyes.' Some of the marine Worms that live in tubes have a number of eyes on the delicate respiratory filaments which project from their anterior end; some have eyes at the hinder end of their body; and *Polyophthalmus* is a veritable Argus, for in addition to the eyes in its head there is a pair on every segment of the body.

Where so much variation is seen it is only natural to expect that external conditions will be found to have very considerable influence. We all know that the Earth-worm is without eyes; and to the various known examples of the reduction of the eyes in such burrowing animals as the mole, and the complete blindness of some that live in caves, such as the blind-fish (*Amblyopsis*) of the Mammoth Cave of Kentucky, and the amphibian *Proteus* of the caves of Carniolia, there have lately been added two blind snails from the just-named Austrian grotto.

The great increase in our knowledge of animals which has resulted and is resulting from the deep-sea explorations which have been carried on with such activity during the last few years, is very strikingly shown in the case of the organ of sight. In one of the earliest of these—that of the Porcupine—a crustacean (*Ethusa granulata*), was discovered living at various depths; specimens taken in shallow water were found to have eyes of the usual character; those which were brought up from depths varying between 110 and 370 fathoms had the ordinary eye-stalks, but the place of the eye was taken by a calcareous knob; the condition was one to which the words of Mr. Darwin may well be applied, 'The stand for the telescope is there, though the telescope with its glasses has been lost.' Specimens of the same species taken from still greater depths (from 500–700 fathoms) exhibited a still more remarkable change: the eye-stalk was found to have lost its special character, and to have been converted into a pointed rostrum. So again a number of new blind Crustacea have been lately described by M. Milne-Edwards from the Gulf of Mexico. Did the matter end here it would be easy enough to ascribe the changes to the disuse of the eye, owing to the want of light at so great a depth. Fortunately, however, naturalists

were spared too hasty a generalization, for from the same depths the dredge of the same ship brought up specimens of the genus *Munida*, in which the eyes were developed to a very great size.

What is true of Crustaceans is true also of fishes, as the following quotation from Dr. Günther will show better than any words of ours: 'The organ of sight is the first to be affected by a sojourn in deep water. Even in fishes which habitually live at a depth of only eighty fathoms, we find the eye of a proportionately larger size than in their representatives at the surface. In such fishes the eyes increase in size with the depth inhabited by them, down to the depth of 200 fathoms, the large eyes being necessary to collect as many rays of light as possible. Beyond that depth small-eyed fishes as well as large-eyed occur, the former having their want of vision compensated for by tentacular organs of touch, whilst the latter have no such accessory organs, and evidently see only by the aid of phosphorescence. In the greatest depths blind fishes occur with rudimentary eyes and without special organs of touch.'*

Interesting as these cases are, and that the more because there still remains to be solved the interesting problem raised by two lines of development being followed by animals living under apparently exactly the same conditions, and starting from exactly the same point, the ordinary vertebrate or crustacean eye, we must turn from them to give an account of a structural arrangement found in various fishes, which will be seen to be not distantly connected with the question of the phosphorescent light of the deep-sea. Of one of these fishes the lamented Willemoes-Suhm wrote home, 'Wie ein leuchtender Stern hing einer im Netz, als er Nachts herauf kam.'

Among the bony fishes (*Teleostei*), and in that division which is distinguished by having the air-bladder communicating by an air-duct with the anterior portion of the intestine (*Physostomi*), and especially among the members of the group which always, or generally, live at great depths, there are sometimes found—as for example in *Scopelus* or *Astronesthes*—one or two rows of small rounded bodies, which are chiefly distributed along the sides of the body, although sometimes, as in the latter of the two just-named genera, they are also very well developed in front of the proper eye. In *Ipnops*, a form first collected by the officers of the *Challenger* exploring voyage, and which is placed by Dr. Günther next to *Scopelus*, there are indeed none of these bodies; but the eye of this fish appears to have lost its function and to have taken on that of a phosphorescent organ.

* To this may be added the case of the Decapod Crustacean, *Willemoesia* (so named after the late naturalist Willemoes-Suhm, of the *Challenger* Expedition); one species of this form was dredged in 1900 fathoms, and the genus is distinguished by having neither eyes nor eye-stalks.

Although it is only quite lately that these organs have attracted any especial attention, their presence in *Scopelus* and other forms was long ago noted by various observers. It was in the year 1839 that the Italian naturalist, Cocco, of Messina, had his attention directed to them, and he published figures and descriptions of the forms which live in the Mediterranean; ten years later a number of the species which possess these structures were treated of in the twenty-second volume of that monument of industry and research, in which Cuvier and Valenciennes systematized our knowledge of the group of fishes. But the work begun by the two, and carried on after Cuvier's death as far as the twenty-second volume, could not, even with its scope, deal in detail with all the parts of every fish; and even had that been possible, the means of investigation which were to hand would doubtless have been insufficient for a complete and correct understanding of the organization of these parts. Modern methods in histology were above all things necessary: even in the case of the human eye, we know that much of what is now the common property of all students resulted from the studies of Max. Schultze in the middle years of the seventh decade of this century. He who will turn to the article on the eye which appears in Stricker's *Handbook of Histology* will see how much has been done since the year 1850; and we have only to compare the first edition of so easily accessible a book as Prof. Huxley's *Lessons in Elementary Physiology* (1866) with the second, which appeared two years later, to recognize at once how all our exacter knowledge of these complicated nervous structures is due to the improved methods of comparatively recent times.

Understanding, then, the real position of affairs, it will be easily seen that the investigation of the minute structure of these eye-like organs offered quite recently an altogether unexplored field of research. With the exception of a short notice by Prof. Leuckart in the year 1865, which led to the belief that the parts in question were accessory organs of sight, nothing seems to have been published with regard to them till the year 1879, when Dr. M. Ussow, of the University of St. Petersburg, gave to the world an account of his researches. These had been undertaken on twelve species, belonging to the seven genera, *Astronesthes*, *Stomias*, *Chauliodus*, *Scopelus*, *Maurolicus*, *Gonostoma* and *Argyropelecus*, all of which are small, and most of which live in the Mediterranean. He pointed out that the organs might be arranged in two series, one of which he regarded as forming accessory eyes, and the other as special glandular organs, and he insisted on the fact that, though they are both arranged in just the same way, the two sets are never found in the same fish.

Before proceeding any further with Dr. Ussow's views, it is necessary to state that early in the year 1880 Dr. Günther gave to the Linnean Society an account of these bodies, in which he expressed his belief that these organs were producers and not receivers of light; and in his introduction to the *Study of Fishes*, published last year, the ichthyologist just named speaks thus in the very last paragraph of his book: 'Many pelagic and deep-sea fishes are provided with peculiar small round organs of a mother-of-pearl colour, distributed in series along the side of the body, especially along the abdomen. Some zoologists consider these organs as accessory eyes, others (and it appears to us with better reason) as luminous organs. They deserve an accurate microscopic examination made on fresh specimens; and their function should be ascertained from observation of the living fishes, especially also with regard to the question, whether or not the luminosity (if such be their function) is subject to the will of the fish.'

One of the required conditions has already been fulfilled. Prof. Leydig, of Bonn, has just published a small volume, illustrated by ten octavo plates, in which he deals in great detail with what he has been able to observe in the spirit specimens of ten species, one of which belongs to a genus (*Ichthyococcus*) not examined by Ussow; to this work we shall shortly return. For the moment the prior studies of the Russian naturalist claim our attention.

All, however, agree generally in their account of the arrangement of these spots; ordinarily they extend from the pectoral fins to the tail, and, according to Ussow, they are covered by scales; sometimes they are found in front of the eye (Pl. VI. fig. 2), on the branchiostegal rays, and on some of the adjoining bones; but these anterior spots never exhibit the same striking regularity of arrangement as do those on the abdomen. They vary considerably in the number present, but an average is found to give the astonishing figure of something like three hundred. In *Chauliodus* Leydig found some thousands of these spots. The most simple seem to be found in *Astronesthes*, a genus which did not come under Prof. Leydig's examination; these have the form of a biconvex lens, which is strongly swollen internally, and has on its outer surface a slight ridge. A layer of connective tissue invests the whole of the organ, and is clothed internally by a layer of brown pigment cells. At the centre of the outer surface there is a small circular orifice, from which the deposit of pigment is absent. Just below this orifice there is a lens, which is regarded by Dr. Ussow as exactly comparable to the crystalline lens of many invertebrate animals; and behind this, and between it and the base of the organ, there is a space of some size which is filled with a watery fluid

('aqueous humour'). On the inner wall of this chamber there is a layer of hexagonal plates, transparent and uncoloured, exhibiting a very regular concentric arrangement, and containing a protoplasmic mass which is longitudinally striated. These plates are looked upon by the author as representing the retina, and as the end-organs of nerve-fibres.

At first sight there can be no doubt that the general characters of an eye are very well represented, but Prof. Leydig, as we shall shortly show, is able to give a very different explanation of the real meaning of these structures.

Let us, however, next take the much more complicated case of *Stomias* (Pl. VI. fig. 1); here the ellipsoidally-shaped eyes are divisible into two unequal portions, the smaller of which is directed forwards, and is applied to the inner surface of the scales, while the other lies among the superficial muscles; the two portions are separated by an extremely delicate partition, and the anterior chamber is filled by an oval and perfectly transparent body (*l*), while the posterior contains a clear and gelatinous substance (*x*), just as in the preceding case of *Astronesthes;* the whole organ is invested by a pigmented layer (*p*), which is only feebly developed on the walls of the anterior chamber, and seems to form, though here we must say we think the resemblance is pressed a little too far, a kind of iris-like diaphragm (*ir*) between the fore and hinder divisions of the organ. It is curious, indeed, that hexagonal transparent cells, apparently made up of prismatic rods, should lie on the inner face of this coloured layer, but we must own that they hardly seem to us to have the distinct retinal arrangement which is assumed for them.

Chauliodus is reported to have larger eye-like organs than the two genera already examined; but it is not necessary to deal in detail with this case, for it appears to us that a mere consideration of the account which is given of it lands us in a peculiarly difficult position. The arrangement of this fish's eye is characterized by the presence of a so-called ciliary body, which is described as being comparable to what obtains in the Cephalopoda.

Here it is most necessary to be exact: are the eyes in question to be compared to those of other Vertebrated animals, or are they to be likened to those of the Invertebrata?

The question is not an unimportant one, for the difference between the two is fundamental and most characteristic, far more important indeed than are most of the distinctions that have been insisted on as justifying that most unfortunate division of the Animal Kingdom into the two great groups of Vertebrata and Invertebrata, which has for long had a disastrous effect on the correct understanding of the real relations

to one another of the more highly developed animal forms; but which, we may say in passing, owes its existence neither to the laws laid down by Cuvier after a comprehensive survey of the adult, nor by Von Baer after a close study of the characters of developing animals.

To this distinction let us devote a little consideration. When we compare a section of the eye of any invertebrated animal with that of one of the Vertebrata, we find, with other subordinate differences, of which nothing need be here said, this most important and striking one; in the Vertebrata the 'rods and cones,' which are the terminal organs of the optic nerve, are placed quite at the back of the eye, are turned away from the light, whereas in the invertebrate, the Crayfish, for example, we find the anterior part of the end-organ just below the cornea, and as near to the light as possible; not turned away from it, but towards it.

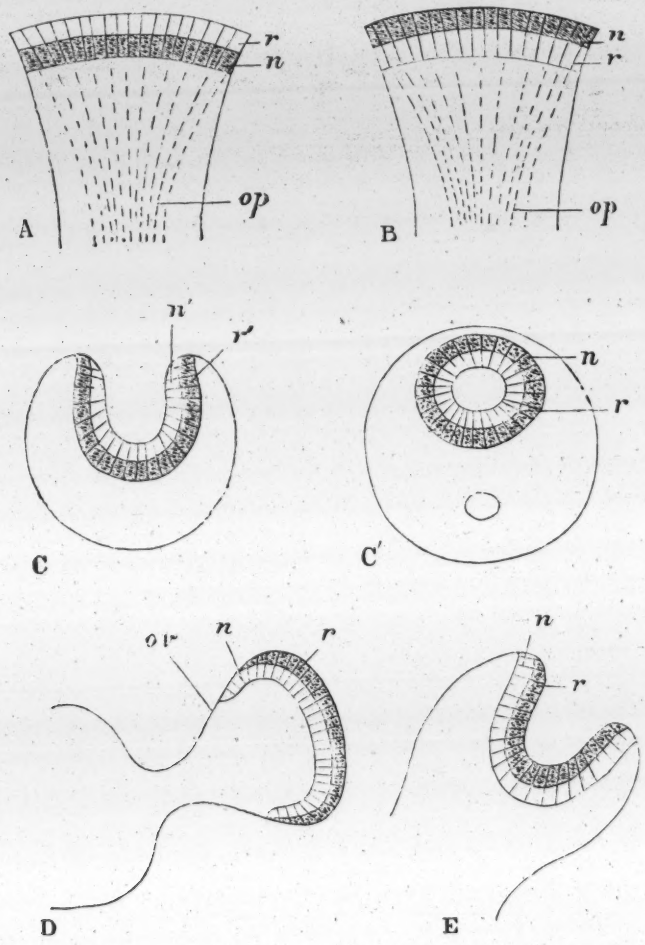
This important difference in the optic arrangements of the adult of the two groups finds, like many other difficult problems, its explanation in the history of the development of the two kinds of eyes. Both have their prime origin in that outermost layer of the embryo—the *epiblast*—which is the veritable mother of all our sensations. In the invertebrate the only change which occurs is that the cells of this layer become modified and elongated; in the vertebrate their history is more complicated; firstly, the layer of epiblast dips in below the level of the back of the body, to form the spinal cord and brain; this set of dipped-in cells get cut off from the exterior, and form a tube of epiblast within the body; from the anterior region of this tube there proceeds an outward growth—the optic vesicle—which grows out towards that thickening of the epiblast which forms the cornea. Thus what was originally outside comes to be inside, and when it grows again to the surface, it does so with its constituent parts turned, as it were, inside out.*

A reference to the subjoined diagrams (p. 228) will perhaps make this difficult point a little clearer. If we take *n* and *r* as representing respectively the layers of nerve-fibres and modified retinal cells, we have in fig. A a representation of what obtains in the Invertebrata and in *B* of what is found in the adult Vertebrate; *C* and *C'* will show the tube of epiblast, while in *D* we see the relation of the two layers in the developing optic vesicle, and *E* shows their position after the 'optic cup' is formed.

The bearing of these considerations is not unimportant; it leads us to ask what is really meant when the spots are spoken

* We owe the first distinct literary statement of this history to Mr. F. M. Balfour, LL.D., F.R.S. (see his *Elasmobranch Fishes*, p. 169).

as 'eye-like organs?' Are they to be compared to those which are found in the head of the Fish, or are they only of the same simple origin as the eyes of the Invertebrata? This question does not seem to have been fully treated of by Dr. Ussow. If they are only to be compared to those eyes or eye-like organs of which we have already spoken as existing in *Polyophthalmus*, the question not unnaturally arises, may they not have some other function? may it not be that their re-



semblance, such as it is, to the ordinary eye, is only another illustration of that instructive and frequently recurring phenomenon that, under somewhat similar conditions, parts of animals, or, indeed, animals themselves, though of the most diverse origin, come to have very similar appearances in their adult condition? The uninstructed naturalist finds it difficult to believe that the wing of a bird and of a bat are formed on totally different structural arrangements; some Crustacea look more like bivalves than Crayfishes; and the remarkable out-

growth from the sides of the skull, which distinguishes the Tortoises, is to be found, to all appearance, in an African Mammal (the Rodent *Lophiomys*), and a lowly Frog (*Pelobates*), found in Central Europe.

Before suggesting any definite answer to the questions thus raised, we must say a few words about the set of organs to which Dr. Ussow does not ascribe any optic function, although he regards them as really belonging to just the same category. As seen in *Scopelus*, they have the form of a pear-shaped sac, the wide end of which lies in the tissues, while the narrower end projects a little above the surface; the latter, which is separated from the part below it by a kind of constriction, contains spherical cells, while the lower portion is filled with cells of a polyhedral form; but there is no central cavity, and there is no kind of orifice. This kind of organ the author looks upon as a gland, and in the more complex case of *Maurolicus* there is no doubt that in what he saw there, there is much to support his view. The 'glands' are elongated and flask-shaped, surrounded for two-thirds of their length by a pigmented membrane; on the inner face of this there is a layer made up of white fibres, and presenting the appearance of a *tapetum*. Here, again, there is no duct to the gland.

We come now to a species which has been examined both by Dr. Ussow and Prof. Leydig—*Argyropelecus hemigymnus*—and it would be interesting, had we space, to compare what these two naturalists have to say about this form. This curious fish (Pl. VI. fig. 2), which is here represented as twice its natural size, is found not uncommonly in the Mediterranean, and is remarkable for the great depth of the anterior portion of its body, and the wonderful way in which it narrows off quite suddenly in the region of the tail. In front of the eye there is one spot, and behind it there are two, and six smaller ones are found on the gill-membrane, and six larger ones are found on the throat; by the gill-cleft we see four, the largest on the body, of which two lie in front and two behind the cleft. At the side of the body, and close to the abdominal profile, there are twelve, which decrease in size from before backwards; and above these there is a second row of six, all of which are of very much the same size. Between the ventral and anal fins there are again four, and behind the anal fin there are six others, of which the smallest are in the middle. Just in front of the tail-fin we find the last quartet of these organs. This gives us fifty-three in all, and as they are paired, we find one hundred and six of these comparatively large spots on the body of this small fish. So much of our account is taken, as is also the illustrating figure, from the description given by Leydig, who would appear to agree so far with Ussow, who

says that in this species there are from one hundred to one hundred and ten of these spots; but they differ in the point of covering scales, the presence of which is asserted by Ussow, while Leydig failed to detect their presence. This, however, is not a point of any great importance, and that the more since we know that in the Sternoptychidae, the family to which this fish belongs, the body is either naked or has only very thin and deciduous scales.

Into the further details, as given by either of the histologists already named, it is impossible for us here to enter; the student who desires to know more of the structural characters will gain a quantity of information from their works.*

If we proceed to sum up briefly the general structure of these organs, we find that while Ussow speaks of them as accessory eyes or as glands, Leydig divides them into three groups; (1) eye-like organs, (2) mother-of-pearl-like organs, and (3) luminous organs; of these the second and third forms would seem to be peculiar to the genus *Scopelus*.

The first set are saccular in form and divisible into a bulb, a neck, and an orifice, and this orifice is always directed downwards. From what has been already said we know that they have an investment of brown pigment, a layer with a metallic glitter, a grey inner body, and a surrounding lymphatic space. The investment is derived from the general integument of the body, and the pigment granules are contained in the cells of the underlying connective tissue; the metallic layer consists of iridescent plates, rods, or fibres. The grey inner body is divisible into two portions, the hinder and larger of which fills the sac, while the anterior and smaller occupies the narrower neck. The striated appearance of this part, reference to which has already been made, is due, in Leydig's opinion, to the presence of a framework of connective tissue, which sends rays into or forms a network in it; into this grey part there further proceeds a nerve, the fibres of which probably come into connexion with the contained cells (see Pl. VI. fig. 5). Ussow, as we know, looks upon these organs as having one of two functions, but never both; Leydig looks at the matter a little differently; without affirming that they are one or the other, he says that whatever they are in *Chauliodus*, to which form Ussow ascribes 'accessory eyes,' that also must they be in *Gonostoma*, where the Russian naturalist finds 'glands;' between the two sets there is, in fine, no essential difference.

* Ussow, M., *Ueber den Bau der sogenannten augenähnlichen Flecken einiger Knochenfische.* Bull. Soc., Moscow, liv. 1879. Pp. 79–115, 4 Pls. Leydig, F., *Die augenähnliche Organe der Fische.* Bonn, 1881. Pp. 100, 10 Pls.

The mother-of-pearl organs (Pl. VI. fig. 4) are, as the figure shows, appropriately named; they have in all cases an outer brown investment, a metallic layer, a gelatinous internal body formed of connective tissue; they are provided with nerves and blood-vessels and are surrounded by a lymphatic space. The metallic plates are regarded by Ussow as special cells, but Leydig looks upon them as altogether similar to the minute iridescent bodies found on the skin. The gelatinous portion is made up of delicate radiate cells, which give rise to a network, and an intermediate soft substance. No essential difference in structure is presented by the so-called luminous organs.

If we look at the bodies from the point of view suggested by Prof. Leydig, it is impossible not to assent to his conclusion that, whatever they are, they are not sensory in function. At first he was inclined to regard them as belonging to that system of sensory parts which runs down either side of a fish's body, and which is seen from without as a well-marked 'lateral line,' but this was a doctrine which a further examination showed him to be utterly untenable.

After these investigations, Leydig proceeds to sum up his objections to the eye-like organs being regarded as presenting anything truly characteristic of the animal eye; and it must be allowed that he makes some of his points with very great force indeed. The examination of *Chauliodus* alone might leave the investigator with a mind inclined towards the explanation given by Ussow, but a clearer view is gained by testing the doctrine by an investigation into the organ of *Argyropelecus;* here (fig. 3) we see that the 'retinal' nerve-cells are connected with the investment formed by the layer of connective tissue; that in fact they are supporting rather than nervous fibres. So again, the brown pigment at first calls to mind an optic organ, but this brown pigment is more widely distributed through the body of the fish, and especially is it developed in the 'frontal gland' or epiphysis of the brain, where also it leaves a central, clear, unpigmented spot. Nor is this all,—the pigment, as our figure shows, is not laid down continuously, as it is in a choroid, but is scattered at various points. The Bonn histologist carries his merciless criticism yet further; the so-called 'lens,' which so easily has ascribed to it the refractive function, does not lie in the centre but at the edge of the mass which does duty as the vitreous body, and, in addition to its position, its histology prevents us from regarding it as a lens.

One possibility remains; it may be argued that the bodies in question are not to be compared to the ordinary vertebrate eye, but that they are nevertheless terminal sensory organs affected by light; but here, unfortunately, we come upon

what is the greatest difficulty of all, the 'pupil,' the 'lens,' and the 'vitreous body' are not turned upwards to the light, but so long as the fish is swimming they look downwards into the deep; and in the case of *Chauliodus* are developed also on the membrane that lines the cavity of the mouth.

After this piece of destructive criticism, it remains for Prof. Leydig to make some suggestion as to what the organs in question can best be compared with. He is of opinion that they bear the closest resemblance to the electric or 'pseudoelectric' organs of other fishes, and he brings many points to strengthen his position. Before we come to any judgment on the matter ourselves, it will be necessary to recall to mind the general structure of an electric organ. These, however much they may vary in details and in their position on the fish that possesses them, always present the following points,—they are richly supplied with nerves, they are surrounded by a layer of connective tissue, which gives rise to a number of 'alveolar' chambers filled with a gelatinous substance. In such a form as the Electric Eel (*Gymnotus*), we find that on the face of each partition which looks backwards, there is a thin homogeneous layer, which is apparently formed of mucous tissue, and on the opposite face, or that which looks forwards, there is a much thicker layer. The nerves passing to and breaking up in this plate, give rise to the electric organ.

When this account is compared with the description that has already been given of the mother-of-pearl organ, we see at once a very considerable resemblance; in both there is a contained mass of gelatinous tissue, in both the same network of connective tissue, and in both nerve-fibres of very much the same character. Nor is this all; in some species, *e.g.*, *Scopelus Rissoi*, there is a spindle-shaped cord or plate, which is formed of extremely delicate granules, and this the author is inclined to regard as comparable to the special *electric plate*. With these essential resemblances, the form is a point of very secondary importance; but even with regard to it Prof. Leydig is ready with an explanation. The rounded form of the mother-of-pearl organs is due to their position on the surface of the body; like the electric organs they would probably become angulated, if they were massed together to form a single body.

Whether they really have the power of developing electricity is a problem that cannot yet be solved; but the question has been put, and it becomes the duty of such naturalists as may have the opportunity to do their best to resolve it.

Nor are we yet in a position to speak dogmatically as to their functions as illuminating organs; no definite phosphorescent organ has ever yet been examined which presented any

other contents than fatty or oily matter,* and the cases are rare in which the part which is phosphorescent has not some other and more important function also; where this is not so—as in the instance of the phosphorescent insects—we find, and it is very curious to note this, that the distinguished histologist Kölliker of Würzburg suggested, nearly a quarter of a century ago, that the luminous organs are essentially nervous and present the nearest resemblance to the electrical organs of fishes. This point is not noticed by Leydig, but it would be curious if it should finally turn out that the organs he has been describing with such care, are intermediate between the other two sets. Even if not phosphorescent, it still remains possible that these creatures add to the feeble light of great ocean-depths by reflecting the light that falls on these eye-like organs. We all know how a cat's eye 'shines' in the dark, and most of us, it is to be hoped, know that this is no case of phosphorescence, but that there is a thick layer of wavy fibrous tissue lining the inner face of the eye which reflects the light; reflecting it, by the way, not for the purpose of illuminating the neighbourhood of a nocturnal beast of prey, but reflecting it for the purpose of as many rays as possible passing to the retina. In some of the fishes of which we have made mention, a somewhat similar tapetum has been found, and it is quite possible that that layer does act as a reflector in one of the worst illuminated regions of the habitable globe.

For ourselves, we are inclined to think that the relations of these organs to those with electric properties in other fishes is the most reasonable that has yet been advanced; they may have a secondary illuminating function of some kind, but it was not the necessity for this that first gave rise to their formation. If we assume the correctness of Prof. Leydig's explanation, we have before us a very interesting example of the way in which scientific hypotheses become theories. An obvious external resemblance to secondary eyes led the first naturalist who studied them closely to look on them as accessory organs of sight; a closer and more detailed investigation has shown that this resemblance is purely artificial, and an appeal is now made not to a seeming likeness, but to a more deep-seated similarity,—to one that depends not on the position of rounded fibrous bodies or to a peculiar arrangement of pigment; but to one, in fine, that depends on community of structure, the first and best implement in any investigation into living objects, and the one that, side by side with the history of development, is the only

* On this point the student is especially to be referred to the various papers of the late Prof. Panceri; for the phosphorescence of fishes, consult 'Panceri' in *Zool. Record* for the year 1871.

criterion to which the judicious naturalist finds it wise or expedient to appeal.

EXPLANATION OF PLATE VI.

FIG. 1. Longitudinal section of eye of *Stomias anguilliformis* × 280, showing the inner vitreous body (*x*), and the anterior chamber with its transparent lens (*l*): *r*, retina; *n*, nerve; *p*, pigmented layer; *ir*, 'iris-like' constriction. (Ussow, fig. 10, *B*.)

FIG. 2. Figure of *Argyropelecus hemigymnus* × 2. (Leydig, i. 5.)

FIG. 3. Longitudinal section of organ from abdominal region of ditto, magnified highly. (Leydig, iv. 25.)

FIG. 4. Two of the mother-of-pearl organs from the side of the body of *Scopelus Humboldti*, seen in reflected light, slightly magnified. (Leydig, vii. 41.)

FIG. 5. Organ from the nasal region of *Ichthyococcus ovatus*, longitudinal section, highly magnified, chiefly to show the entrance of the nerve. (Leydig, vi. 33.)

THE BLACKHEATH SUBSIDENCES.

By T. V. HOLMES.

MANY readers of the *Popular Science Review* have doubtless heard of the strange subsidences that have appeared on the surface of Blackheath during the last three years, and have attracted so much attention from the suddenness of their appearance, and the mystery surrounding their origin. For when it is remembered that these subsidences suddenly disclosed themselves in the midst of a gravel plateau, every foot of the surface of which is liable to be tested by cricket, golf and football players, schoolboys and holiday-makers, it is evident that nowhere could they have been more unlooked for and alarming.

Having been a member of the executive branch of the committee formed for the purpose of investigating one of these remarkable holes, and the exploration being now finished, I am enabled to give some account of our labours and their results. In addition, I shall review the evidence bearing on the subject, and point out what seems to me to be the most probable conclusion derivable from it. Perhaps the best plan will be to begin with a sketch of the geology of the neighbourhood, a clear understanding of which is absolutely necessary, before giving the details of our workings. The archæological evidence need not be taken into consideration till the geological evidence has first been fully weighed.

The voyager from London to Woolwich cannot fail to notice the range of wooded heights on which Greenwich Observatory stands, which contrasts so much with the low ground bordering the Thames in other directions. This range is flat-topped, and is bounded on the east by the valley of the Darent, and on the west by that of the Ravensbourne; Blackheath occupies the promontory at its western end. At the base of this wooded range lies the Chalk, which is well exposed in

the neighbourhoods of Charlton and Lewisham, though elsewhere, for the most part, hidden from sight by the agency of faults or surface deposits. Above the Chalk, along this escarpment, lies the Thanet Sand; then come the Woolwich beds; and, lastly, the Oldhaven or Blackheath pebble beds crown the plateau. Further inland, at and around Shooter's Hill, the London Clay exists in considerable thickness, and extends westward between Kidbrook and Eltham in a slight synclinal fold. At Eltham, the surface is again composed of the Woolwich and Blackheath beds. The newest beds in the district immediately around Blackheath are the gravel capping Shooter's Hill, which is said by Mr. Whitaker * to be intermediate in age between the Lower Bagshot beds and the Boulder Clay, and the gravel and alluvium of the Thames Valley.

A glance at the Geological Survey map, shows that the Blackheath pebble beds and the Woolwich beds crop out on the northern, western, and southern sides of Blackheath. To these may perhaps be added the Thanet Sand, which, though it is shown on the map only on the north and west, and just round the south-west corner, was seen here and there by Mr. Prestwich† at the bottom of the cutting between Blackheath station and tunnel; and I was informed by Mr. Chapman, station-master at Blackheath, that the station there is founded on what can only be Thanet Sand. The dip of the various beds at Blackheath is very slight in amount, and is southerly in direction. In Westcombe Park (east of Greenwich Park) and the adjacent railway cutting, they are very well shown at heights only 15 to 20 feet above their level in the cutting west of Blackheath station, though the distance, measured along a line ranging due north and south, is about a mile and a furlong. It will be seen, therefore, that the geological evidence at Blackheath is much greater in amount than usual, and that it tends to show that the beds are lying nearly level, and are free from faults or disturbances of any appreciable magnitude.

There is evidence, in the appearance of certain spots on Blackheath, that holes like that to be described have disclosed themselves from time to time, and been filled up without any curiosity having been (apparently) excited as to their origin. But in April, 1878, the sudden appearance of a cylindrical hole, about 20 feet deep and from 7 to 8 feet in diameter, made more impression on the minds of the inhabitants of the neighbourhood than had previously been the case. The hole was, however, after remaining open a few weeks, filled up by

* Whitaker, *Guide to Geology of London*, p. 56 (3rd edit.).

† *Quart. Journ. Geol. Soc.* vol. x. p. 104.

the Metropolitan Board of Works, without any attempt at exploration. In November, 1880, two other holes suddenly appeared, and the deeper of these two holes is that the examination of which has just been concluded. The Metropolitan Board having stated, in answer to the Astronomer Royal's request that they should explore the first hole, that they had no funds for that purpose, the matter was taken up by the Lewisham and Blackheath Scientific Association, the then President, Mr. J. K. Laughton, R.N., and Mr. H. W. Jackson, F.R.A.S., the Hon. Secretary, being especially zealous in the matter. A small sum was voted by the Association for the exploration, but was soon exhausted; and money coming in but slowly from the neighbourhood generally, April had arrived before the Subsidence committee had received sufficient funds to enable them to continue the work.

The hole which appeared in April, 1878, and that which has just been examined, were almost identical in size and appearance. The last was, when first seen, like a nearly circular shaft with vertical sides, and 18 feet deep.* It was in its longer diameter 7 ft. 8 in., and 6ft. 9 in. in the shorter. When the fallen earth from the bottom was removed, the sides were found to recede till the hole became about 14 feet in diameter, at a depth of about 22 feet. The third hole, that near Eliot Place, was shallower than either of the other two, and somewhat different in shape. The difference, however, was chiefly one of proportion, the shaft-like portion of the hole being very short, and its sides receding more rapidly than in the other two. The Eliot Place hole, after remaining open some months, was filled up by the Metropolitan Board of Works towards the end of April.

During the first brief exploration of the hole, the heap of material at the bottom was carefully examined by Mr. Jackson during its removal, and found by him to correspond exactly to the sand and clay which formed the sides of the hole at and for 7 feet below the surface. On recommencing the work in April, the first shaft, 11 ft. 3 in. by 9 ft., which had been carried to a depth of 16 feet, was considered unnecessarily large, and consequently expensive; and the work was carried on below that depth by means of a shaft 6 ft. by 5 ft. 6 in. Below the surface-material were found Blackheath pebble beds of the ordinary type and orange colour. At a depth of 34 feet water appeared, and continued to fill the hole to about that level during the progress of the excavation, so that a pump became necessary. From 36 to 42 feet, whitish sand and pebbles were brought up. The proportions of sand to pebbles varied considerably, and occasionally differed in a marked

* See *Engineer*, Feb. 4, 1881.

manner at different sides of the shaft. The ground below appeared to be loose and disturbed, as was shown by the ease with which an iron rod could be thrust downwards to its full length of 8 feet, compared with the variable but always much greater difficulty attending a horizontal thrust. At the depth of 42 feet, the timber framework of the lower shaft began to settle down on one side very markedly. This settlement was down to the north and north-west, the southern side resting apparently on more solid ground. As the funds at the service of the Subsidence committee were getting very low, and further excavation promised to be a work of great expense if not of danger, it was resolved to cease digging and to try the following plan, which was proposed by Mr. Bond, one of the most active and useful members of the committee.

Gas-pipes, with an internal diameter of 1½ in., were screwed together and driven down vertically from the bottom of the shaft. All solid matter was kept out of these pipes during their descent by means of a plug with a harpoon-like point, which fitted into the lowest end; the base of this point was of somewhat greater breadth than the external diameter of the pipes. Having reached a depth of 85 feet from the surface, the pipe was raised a few inches and the plug dropped out; then a slender rod, with a kind of spoon at its lower end, was passed down the pipe, and a spoonful of the stuff just outside its lower end brought to the surface. As the pipe was withdrawn, spoonfuls were brought up at intervals of a few inches, so that samples of the material just outside the pipe at various depths between 42 and 85 feet were obtained. Both on driving down and withdrawing the pipe, the resistance encountered varied exceedingly, a progress of 13 feet in one day being followed by one of as many inches the next. Yet there was but little variation in the nature of the material extracted; and it seems likely that the occasional presence of hard nodules, such as may be sometimes found below the well-known shell beds of the Woolwich series, was the cause of the occasional slow progress.

The stuff brought up in the spoon was, between 42 and 50 feet or thereabouts, brown or greenish-brown sand, with but little clay, and occasionally a small pebble. Below 50 feet, no pebbles were brought up, and the proportion of clay to sand increased with the depth, though not with perfect regularity; so, also, did the proportion of dark blue clay to brown. No fragments of shells could anywhere be detected.

It will be well now to consider what evidence we have of the thickness and character of the various beds at Blackheath, and the bearing of this evidence on the results of our exploration. At the house next to the Sun-in-Sands, Shooter's Hill

Road, about three-quarters of a mile E.N.E. of the hole, the Chalk was found to be 109 feet below the surface,* the height of the surface being about 150 feet. The surface at the hole is about 132 feet above Ordnance datum, which would give 91 feet as the probable depth of the Chalk there, granting it to be at the same level in the two places, which is probably very nearly the case. About a mile W. of the hole, at Blackheath Hill, the top of the Chalk appears to be about 50 feet above Ordnance datum, which would give about 80 or 82 feet at the hole. The Thanet Sand, which directly overlies the Chalk, was 55 ft. 10 in. at the Brewery at Greenwich Hospital (now Naval College), but at Loampit Hill, Lewisham, is only about 40 feet thick. The Woolwich beds, at Blackheath, judging from the sections now to be seen at Westcombe Park, and from those seen by Prof. Prestwich in the cutting east of Blackheath Station, are a little over 20 feet thick. Greenwich Hospital Brewery gives 23½ feet.† The thickness of the pebble beds which form the surface of Blackheath is, of course, variable.

In Westcombe Park the Woolwich beds are not fully seen, but the shell beds are about 5 ft. 6 in. thick, and below them is sandy clay containing nodules of sandy ironstone. It may be interesting to compare the section at Greenwich Hospital Brewery, with that seen by Mr. Prestwich east of Blackheath Railway Station.*

Blackheath Railway Cutting.

Blackheath pebble beds, 10 to 12 feet.

		Feet.	
Woolwich beds.	Brownish sand	2	
	Broken-up shells in light-coloured clay, with pebbles	4	Shelly beds.
	Broken-up shells in greenish-grey clay	2	
	Light green sandy clays, mottled red and grey	7	
	Light greenish sands with a few pebbles (bottom bed)	6	
Thanet Sand		21	

Greenwich Hospital Brewery.

Blackheath pebble beds.

		Ft.	In.	
Woolwich beds.	Black sand	4	10	
	Blue clay	0	8	Shelly beds.
	Shelly rock	4	0	
	Red clay	6	0	
	White sand	4	0	
	Green sand and pebbles	4	0	
Thanet Sand		23	6	

* Whitaker, *Geol. Lond. Basin*, p. 457. † Ib. p. 465. ‡ Ib. p. 124.

In Westcombe Park, water comes out along the line of outcrop of the shelly beds; and Mr. Chapman, station-master at Blackheath, informed me that water, evidently from the same source, affords a supply for the railway station. The water is that which, falling on the permeable pebble beds in the form of rain, is upheld by the impermeable clay of the shell beds below. The shell beds were cut through in making Blackheath tunnel, and the water held up by them keeps the tunnel constantly wet. There can be no reasonable doubt that the water found at a depth of 34 feet in the hole, and which afterwards rose to 33 ft. 1 in. after rains, was water from the base of the pebble beds, and indicated the close proximity of the shell beds below. In the Vale of Health, Messrs. Jackson and Bond found that water was met with at a depth of from 12 to 14 feet. Now as the average height of the surface at the Vale of Health must be about 18 or 19 feet below that of the surface at the explored hole, more than 400 yards N.E. of it, we have herein evidence that the water in both places is from the same source, and that the shell beds lie at each spot at very nearly the same level.

The geological evidence already given shows decisively that the various spoonfuls of material brought up from the hole cannot represent the nature of the beds existing below the surface of Blackheath in undisturbed ground. And we have also learned from it how exceedingly improbable is the existence of any faults or disturbances of the strata there. For while we have more evidence than is usually available in areas of similar size tending towards the last-mentioned view, it is apparent, from the sections given of the constituents of the various beds, that the only clayey beds belong to the Woolwich series and are about 12 or 13 feet thick at the utmost, whereas, judging from the contents of the spoon, clayey beds exist of a thickness of more than 30 feet. In what way, then, can this apparent thickness of clay be accounted for?

The sticky nature of clay, and the small quantity of material brought up may have had some influence in this result, but will not, I think, fully account for it. We have now, in fact, arrived at a point at which it becomes necessary to consider the two views which may be taken as to the origin of these Blackheath subsidences, and their respective probabilities. The first, which may be called the geological view, supposes them to have arisen from the breaking in of the roof of caverns in the Chalk formed simply by the action of water, which, percolating through the overlying tertiary beds, has dissolved away the Chalk in the manner so often seen in the 'sandpipes' of chalk-pits and railway cuttings. The second, or archæological view, is that these subsidences are the result of the action of water on

an old shaft leading to a cavern formed (probably in the chalk) by the hand of man. A third view appears to be entertained, which I mention for that reason only, though it has neither evidence, nor even possibility, in its favour. It is that 'the Blackheath subsidences are due to the action of water, the sand being carried off and leaving the pebbles to collapse.' (*Times*, May 12, 1881.) The holders of this view appear to forget that the formation of *natural* underground cavities in highly siliceous sand and gravel is impossible, inasmuch as they, unlike the Chalk, remain utterly insoluble when exposed to the action of rain-water charged with carbonic acid gas. I need therefore say no more about this third view, but will consider the opinion first mentioned, the only geological view which deserves the name.

Allowing 35 feet as the depth at which the top of the shell-beds would be touched, in unbroken ground near the hole, the beds thence to the top of the Thanet Sand must be quite 18 feet thick. The Thanet Sand cannot be less than 40 feet thick, and may be 5 or 10 feet more. Thus, at the lowest computation, the distance to the Chalk would be 93 feet. Mr. W. Whitaker, however, who has probably had a more extensive experience of these beds than any other living geologist, remarks that while a few feet only of Tertiary beds above the Chalk give rise to an uneven surface, and to pipes, the same process of the dissolving of the Chalk by carbonated water 'when it goes on at greater depths seems to act in a more equal (because in a more constant) way on the surface of the Chalk, leaving it even.'* Now Mr. Whitaker's experience must have been gained in pits and railway cuttings that would seldom show even 50 or 60 feet above the Chalk, much less 80 or 90 feet. Again, Mr. De Rance observes† that the permanent water-level in the Chalk at Blackheath is about Ordnance datum, and that the comparatively small amount of chalk there above this water-level makes it very unlikely that these subsidences are due to pipes descending vertically into the Chalk. Lastly, our own experience of the way in which water falling on the surface of Blackheath is held up by the shell-beds below, and thrown out just above their line of outcrop, is conclusive evidence that scarcely any water can ever get down to the Chalk below at all. All that does do so (artificial holes apart), must fall as rain outside the area of the Blackheath plateau.

An interesting letter appeared in the *Engineer* of Feb. 18, 1881, signed Alfred W. Morant, in which the writer gave an account of some remarkable subsidences which appeared in great numbers many years ago on the sewage farm at Whitlingham, near Norwich. He says: 'When the sewage was first

* *Geology of London*, 3rd edit. p. 32. † *Nature*, Feb. 17, 1881.

allowed to flow over the land, we were astonished day by day to find the fields to be soon covered with circular holes, usually about 3, 4, or 5 feet in diameter and of various depths, the sides always being vertical,' &c. Mr. Morant attributes these subsidences to the excessive quantity of carbonated water suddenly poured over the land, and its effects on the numerous sandpipes which exist in the Chalk of that neighbourhood. On visiting the Whitlingham sewage farm at the end of last March, I saw many of these holes. Those I saw were seldom more than 2 or 3 feet deep, but I was told by the old man who was then engaged in distributing the sewage over the surface, that they often on first appearance were 10 or 12 feet deep, or even more, but that they speedily filled themselves up, to the depth of those I saw, by the tumbling in of their sides. They are certainly very singular and worthy of notice, and are doubtless due to the cause assigned by Mr. Morant. But at Whitlingham we have an immense quantity of carbonated water making its way through loose, highly porous, sandy and gravelly beds, having a thickness of perhaps from 10 to 30 feet, to highly disturbed and probably fissured chalk below.* Another thing which seems to have had, in all probability, a special influence on the production of subsidences at Whitlingham is this. Among the strata overlying the Chalk, there is a bed of pebbles cemented together by oxide of iron and manganese, known locally as 'iron-pan.' Where a sandpipe occurred in the Chalk below, the superior hardness and tenacity of the iron-pan would often be the means of preserving it unbroken and horizontal, while the softer and looser beds beneath the pan had adapted themselves to the shape taken by the sandpipe. A hollow space, shaped like an inverted cone, would be accordingly here and there formed beneath the iron-pan. The form taken by the subsidences at Whitlingham seems to me, therefore, to have been the result of the shape of the hollows probably existing below the pan, which became suddenly filled up by the disintegration of the pan through the influx of sewage water. The anticlinal existing in the Chalk at Whitlingham may very probably reduce the maximum thickness of the overlying beds at the sewage farm to 20 feet or less. But, at all events, the differences between the Blackheath and Whitlingham subsidences must be sufficiently obvious. This Whitlingham case seemed to me, however, well worth examination here, because it is the only one I have met with at all resembling that of Blackheath, which is best explained by the geological theory.

We now come to the archæological theory of the production of the Blackheath subsidences; and perhaps the best plan will

* See Reports of Papers by Dr. J. E. Taylor, on the 'Beds at Whitlingham.' *Geol. Mag.* for 1865 (vol. ii.), pp. 324 and 370.

be to give a general account of the archæological evidence bearing on the matter, and then to discuss its applicability to the case before us.

Artificial shafts in the chalk occur in great numbers in parts of Norfolk, Suffolk, Kent, and other counties where chalk abounds. They naturally divide themselves into two groups: those made for the purpose of obtaining chalk and flint (mainly the latter), of which 'Grime's Graves,' in Norfolk, about three miles N.E. of Brandon, furnish by far the best known examples: and those made as places of security, and of warmth in winter, of which the 'Danes' Holes' of Crayford and other places in Kent, near the south bank of the Thames, are the most noteworthy instances.

The ancient pits known as Grime's Graves have been fully described by the Rev. Canon Greenwell, and from his paper on the 'Opening of Grime's Graves,'* I will make a few brief extracts, sufficient to show their general character, and refer those desirous of more details to the paper itself.

The pits are about 254 in number, and are generally about 25 feet apart, covering a space from 20 to 21 acres in extent. They are circular, and vary in diameter from 20 to 65 feet. They have all been filled up to within about 4 feet of the surface. The pit selected for examination was about 28 feet in diameter at the mouth, and gradually narrowed to a width of 12 feet at the bottom, which was 39 feet below the surface. It was cut through a deposit of dark yellow sand, 13 feet thick, which there overlies the Chalk. At the depth of $19\frac{1}{2}$ feet from the top of the Chalk was a stratum of flint of better quality than any nearer the surface. This is called by the present workers —for in this neighbourhood the manufacture of gun-flints still goes on — the 'wall-stone,' and is used for building walls,† though not hard enough for gun-flints. At a depth of $7\frac{1}{2}$ feet below the wall-stone, or 39 feet from the surface, a second bed of flint exists of much better quality, which was, and is now, used for gun-flints. It is probable that the ancient workings were established in this place on account of these qualities in the flint. At the bottom of the shaft this bed of flint, known now as the 'floor-stone,' was worked out as far as the shaft extended, and then galleries were driven out in various directions upon the level of the bed of flint, the overlying chalk being removed to a height of 3 to 5 feet. The galleries vary in width from 4 to 7 feet, and the flint was worked out beyond their sides as far as was practicable without causing the roof to give way. These galleries probably connect all the shafts. There were no steps cut in the sides of the pit, and the workmen either

* *Journ. Ethnological Soc.* Jan. 1871.

† See Mr. S. B. J. Skertchly's *Geological Survey Memoir on Gun Flints.*

were drawn up by ropes, probably of hide, or ascended by means of a ladder, which would be most probably made by cutting notches in a tree stem. The principal instrument used, both in sinking the shaft and working the galleries, was a pick made from the antler of the red deer, many examples of which were found. When a new pit was sunk, the material excavated went to fill up an old one.

Ancient pits of the Grime's-Graves type must be carefully distinguished from certain modern shafts which exist in considerable numbers, and are for the purpose of obtaining chalk where it is some distance below the surface, and yet required for economical purposes. Mr. Aubrey Strahan of the Geological Survey, informs me that there are many such shafts in brick-yards in the country west of Windsor; and Mr. H. B. Woodward has lately called my attention to a paper by the late Dean Buckland on the 'Plastic Clay'* (the old name for the Woolwich beds), in which mention is made of a shaft of this character at 'the base of the north-east extremity of Shooter's Hill.' Buckland states that 'in the same field with the clay-pits, and on the north side of them, a shaft is sunk 120 feet to the surface of the subjacent Chalk, which has been extracted to the further depth of 24 feet, being the object for which the shaft was made. He also gives instances of the way in which the 'Plastic Clay' upholds the water in the district, and throws out a line of springs at its outcrop.

Pits of the kind known as Danes' Holes, abound in the neighbourhood of Dartford, Crayford, and Bexley, and near the village of East Tilbury, on the northern bank of the Thames. Similar excavations are said by the author of Murray's *Handbook to Kent* to exist on either bank of the Somme as high as Peronne, in the diocese of Amiens. The tradition of Picardy asserts that 'these caverns were used for the retreat and concealment of the inhabitants in time of war, whence their ordinary name, *les souterrains des guerres*.' Those in the neighbourhood of Crayford are thus described in Hasted's *History of Kent*, vol. i. 1778: 'There are now to be seen, as well on the heaths near Crayford as in the fields and woods hereabouts, many artificial caves or holes in the earth, some of which are 10, some 15, and others 20 fathoms deep. At the mouth, and thence downward, they are narrow, like the tunnel of a chimney, or passage of a well, but at the bottom they are large and of great compass, insomuch that some of them have several rooms or partitions one within another, strongly vaulted and supported with pillars of chalk.' Hasted remarks that many of those on Dartford Heath 'have been stopped up of late,' to prevent the frequent accidents from men and cattle

* *Trans. Geol. Soc.* 1st series, vol. iv. p. 277.

falling into them, and refers to the *Germania* of Tacitus, who describes the Germans as digging out subterranean caves. 'They pile upon them great heaps of dung, as a shelter from winter and as a receptacle for the year's produce, for by such places they mitigate the rigour of the cold. And in case an enemy approaches, he lays waste the open country, while what is hidden and buried is either not known to exist, or else escapes him, from the very fact that it has to be searched for.' (*Transl. Church and Brodribb.*)

Hearing that Mr. F. C. J. Spurrell, of Belvedere, was the chief authority on the subject of these Danes' Holes, I wrote asking where they might best be seen in the neighbourhood of Bexley, having always thought that the Blackheath subsidences were the result of artificial excavations of some kind. Mr. Spurrell replied that Joyden's Wood was a good place, and that he thought the Blackheath subsidences would be found to result from the presence of Danes' Holes there. On the 7th of April he read a very interesting paper on Danes' Holes at the Archæological Institute, in which he stated that they were known to exist as far west as Charlton Park. A remarkable pit, probably originally a Dane's Hole, was discovered in February 1878 at Eltham Park,* and a strange subsidence occurred five years ago in Kidbrook Park Road, on the eastern border of Blackheath. And it is almost certain that the extensive cavern under the Point at the north-west corner of Blackheath should be referred to this class, though it was entered by a gallery driven into the hillside, and not by means of a vertical shaft.

The pits of Joyden's Wood are most abundant in that part called Cavey's Spring. The shafts seen by me were all in Thanet Sand, and were very narrow, though, of course, considerably broader at the mouth than they were five or six feet lower down. I do not think that, except just at the mouth, they were more than 2 feet 9 inches in diameter. Two of them probably communicated with each other below, as they were not more than 80 feet apart. The ground, indeed, for a space of 4 or 5 acres, is covered by bell-shaped pits 8 or 10 feet deep, while here and there appears one of these shafts, sometimes from the bottom of a bell-shaped pit, sometimes not. I was informed that these shafts sometimes appeared suddenly and unexpectedly, and that they were about 70 feet deep. The pit at Eltham Park is 140 feet deep, and has (probably since it was first made) been lined with courses of brick and chalk, so that its present diameter is from 4 feet 1 inch to 4 feet 2 inches. A bed of flint forms the roof of the chamber, which

* See Paper by Mr. W. M. Flinders Petrie, *Proc. Roy. Archæol. Inst.*, March, 1878; and for plan and section of pit, *Engineer*, March 18, 1881.

was found, on discovery, to have been used for a considerable time as a cesspool. The existence of both shaft and chamber was unknown to Mr. Jackson, the present proprietor of Eltham Park, till their discovery at the end of a disused drain by a workman sent along the drain to find out the course taken by a quantity of water that had escaped through a leakage. Mr. Petrie notices that five courses of chalk occur in the shaft in the midst of the brickwork, about 47 feet from the surface, and remarks that the chalk for these courses was no doubt procured from the chalk in the shaft 70 feet below, which must have been unlined at the time from these chalk courses downwards. It seems to me most likely that the shaft was first lined when the chamber below was utilized as a cesspool, and that it is impossible that the latter could have been originally constructed for that purpose.

It will be evident that the Danes' Holes mentioned differ from the other classes of pits in the Chalk not merely in details of construction, but in geological position. For the others are sunk where chalk is at or close to the surface, as at Grime's Graves, or in brickyards, as at Shooter's Hill. But though pits of the Danes'-Hole class are not necessarily found on any one particular horizon, a little consideration shows that the pits at Joyden's Wood, Eltham, and Charlton Park cannot have been intended as pits for chalk or flint. East of Joyden's Wood is a broad spread of bare chalk, yet the makers of the pits there have preferred to go through 40 to 50 feet of Thanet Sand in addition; while Eltham Park, Charlton Park, and Blackheath, all stand on the Blackheath Pebble Beds, and could never have been the sites of brick or tile works. In addition, there is plenty of chalk near at hand, both at Charlton and Blackheath, north and west of the plateau.

Thus by a process of elimination ending in the 'survival of the fittest,' pits of the Danes'-Hole class remain as the only probable explanation of the Blackheath subsidences. A little consideration will show that the inconvenience as regards the water at 34 feet, which was so serious a matter to us, with our large shaft and shattered ground, would not be very formidable to men sinking a shaft of 2 feet 9 inches in unbroken material. A relative who has seen much of primitive mining in Bolivia at once suggested to me that they would soon stop the percolation of the water by claying the sides for the two or three feet in which such protection was necessary. The pebble beds would probably stand very well in the shaft if subject only to the gentle percolation of water, and shielded from the direct action of rain. Of course a shaft in such strata would be ascended and descended by means of a notched tree-trunk, or

of two or three spliced together, as I am informed is the usual mode in Bolivia.

It seems likely that the upper third of the narrow shaft became filled up mainly by the falling in of the pebble beds, leaving the lower two-thirds almost or quite empty. (Possibly a very slight hollow without any outlet, about 30 feet in diameter, and perhaps 1 ft. 6 in. deep at the centre, which exists on the west side of the hole at the surface, marks the amount of the material that thus filled the upper part of the shaft.) And a little reflection shows that the line of junction of the pebble beds with the clay beds below, is the spot at which the incessant trickling of water down the neglected shaft could not fail to produce, sooner or later, subsidences precisely resembling those which have appeared; for the effect of the water concentrated towards the base of the pebble beds, would be to undermine them and wash the disintegrated sand and pebbles down the shaft, together with a considerable but smaller amount of the subjacent clay. In this way a hollow would gradually be formed, conical in form, from the base of the pebble beds upwards, while the mouth of the shaft below would acquire a broad funnel shape, from the denudation of the upper surface of the clay. The Thanet-sand and chalk part of the shaft would probably be little altered, and the chalk cavern below would allow of the percolation through it of any quantity of water, and be at the same time more than spacious enough to receive any quantities of sand, gravel, or clay, the removal of which would suffice to cause a subsidence at the surface such as we have endeavoured to examine.

Note.—As the Subsidence Committee are by no means unanimous with regard to the origin of these holes, it has been resolved that in their forthcoming Report the various opinions held shall all be represented.—T. V. H.

SCIENTIFIC TEACHING.

By W. H. STONE, M.B., F.R.C.P.,
Formerly Scholar of Balliol College, Oxon; Lecturer on Physics at St. Thomas's Hospital.

IT is doubtful whether the generality of well-educated men fully appreciate the great, the radical, and the almost revolutionary change which has in the past thirty or forty years come over the scope and spirit of English liberal education. Indeed it can hardly be termed a change; but might be more correctly designated as a substitution of one branch of human knowledge for another. For whereas in the first forty years of the present century, the dead languages, especially Latin and Greek, History, Logic, and Metaphysics, fairly held their own against the computative sciences of Mathematics, Mechanics, Physics, and Chemistry, and the systematic or classificatory subjects of Botany, Geology, and Zoology as topics of teaching and examination; they seem at the end of the second forty to have been all but superseded. No doubt in the main the revolution, great as it undoubtedly is, has proved salutary. Englishmen, with their characteristic tenacity of existing forms, had retained all but unchanged in their large public schools and in the older universities a form of intellectual culture which really originated in the Middle Ages, or at the latest with the restoration of learning. This is no mere figure of speech. The writer of the present remarks took his first childish lessons, after mastering the rudimentary arts of reading and writing, from *The Boke of Roger Ascham*, and received his first rewards for saying, parrot-like by rote, the ancient farragos now only known by their initial words,—'Propria quæ maribus,' 'Quæ genus,' and 'As in præsenti.' Of the present generation, not one in a thousand has ever even heard of these mediæval *aide-memoires*, or of the somewhat more useful Scholastic Scheme of Syllogisms, beginning with the cabalistic formula, 'Barbara Celarent.' Later on, he and his companions were expected weekly to manufacture,

nolentes volentes, a certain quantity of poetry!—God save the mark!—in the Latin and Greek tongues. He can well remember his father's remonstrance on finding him working at 'that nasty chemistry, when you have not done your Latin verses.' Perhaps the most singular travestie of teaching was the inculcation of that laboriously useless heap of conflicting rules termed the 'Greek Accents.' It was well known to every scholar that they were non-existent in classical times; that they were probably prosodiacal; that they sprang up about the time when Greek was going out of use as a spoken language; and that, except in very few instances, they now served no purpose whatever. In spite of this, they were steadily and perseveringly thrust down the throats of schoolboys, insomuch that ignorance of the hideous pedantry of a mediæval grammarian might involve the pain and humiliation of corporal punishment.

That all, or most, of this has been swept away is ground for unmixed satisfaction. But it does not absolutely follow that what is being substituted for it is beyond comment or improvement. There may be errors and pedantries developing in the new as in the older system. Nor are they difficult to point out.

The teaching of science has tended to give an impulse to the computative, to the disadvantage of the judicial and appreciative functions of students' minds. Indeed the computative faculty, so highly developed at times in men not otherwise liberally educated, is not the widest in intellectual scope, nor the fittest preparation for some branches of life-work. Men in after-life are called upon to use their imaginative powers, to sift evidence, and to weigh symptoms, as well as to solve problems. They may adopt artistic or literary pursuits, they may choose the professions of Law or of Medicine. In all these, the attempt to reduce the subject-matter laid before them to the strict conditions of an equation or a ratio, so far from being a fruitful mental effort, may absolutely prove a hindrance. There is a common type of mind which fails to see a proof which is not of the character of demonstration, and which, in its absence, neglects to use the faculty of judgment and decision so necessary in the common affairs of business.

The computing school, and especially those who teach its Physical branches, very correctly and consistently insist upon the solving of problems as a test of thorough knowledge. Mr. Day, whose work appears to be mainly performed 'in the laboratory of King's College, under the direction of Professor Adams,' in an excellent collection of questions upon Electrical Measurement, says:—'It is now universally admitted that numerical exercises are necessary in the study of the experimental

sciences, both as giving practice in the application of the various theories, and as affording tests of ability to comprehend as well as to apply that which has been learned.'

It must be remembered, however, that even among advanced and professed mathematicians, the faculty of solving problems is very unequally distributed; a fact which is openly recognized at the great mathematical University of Cambridge. The problems themselves are often open to comment, as partaking of the nature of enigmas, or riddles, rather than as fair tests of knowledge. Like riddles, moreover, they exercise a kind of fascination on their concoctors, and are very liable to figure in papers of questions. The writer, for instance, has seen in a paper on Physics a question which involved an indeterminate equation, and of which the solutions were infinite in number. Surely this should have been relegated to its kindred Algebra. But an instance which has occurred within the present year is so exceptional as to deserve quotation. It was a Pass, not an Honours paper, set for Matriculation,—the primary and initial step of the whole University career; a gate to further knowledge, which should be prudently left as wide open as is consistent with a reasonably high standard. The paper consisted in all of sixteen questions, and is therefore too long for quotation in full. Of these, says the heading,—

'*Not more than Eight Questions are to be answered, of which at least Two must be selected from Section A.*

A.

'1. State your reason for regarding a pound as a unit of mass and not of force. What is the most convenient unit of force when a foot, a pound, and a second are units of length, mass, and time, respectively?

'2. State the conditions necessary for the equilibrium of a body free to move in one plane. To what do these conditions reduce when one point in the body is fixed?

'3. A solid right circular cone of homogeneous iron is 64 inches in height, and its mass is 8192 lbs. The cone is cut by a plane perpendicular to the axis so that the mass of the small cone removed is 686 lbs. Find the height of the centre of gravity of the truncated portion remaining, above the base of the cone.

'4. A heavy body starting from rest slides down a smooth plane inclined 30° to the horizon. How many seconds will it occupy in sliding 240 feet down the plane, and what will be its velocity after traversing this distance? [$g = 32$.]

'5. What is the "Kinetic Energy" of a moving mechanical system? A shot of 1000 lbs. moving at 1600 feet per second

strikes a fixed target. How far will the shot penetrate the target exerting upon it an average pressure equal to the weight of 12,000 tons?'

If it be borne in mind that judgment on the five momentous mathematical generalizations (for they are hardly within the pale of Physics proper) was demanded of boys averaging sixteen or seventeen years of age, fresh from school, it will be evident that the race of schoolmen and of De Morgan's 'Conundrum'-makers is not yet extinct, and that the current rumour of the award having been returned to the examiners for mitigation may have some foundation in truth.

It is interesting to note how this radical change in the scope and subjects of education has reacted on our older and on the more recently founded Universities. Far in the van stands that of Cambridge. Here, from the traditional character of the instruction given, little modification was required to bring modern requirements into harmony with the older teaching. Ever since the appointment of the great author of the *Principia*, the discoverer of the Binomial Theorem, and of the 'Fluxionary Calculus' to a Junior Fellowship in Trinity College, A.D. 1667, Physics and Mathematics have had their full and abundant share in the curriculum of this University. If, therefore, there has been a greater leaning towards Physics and Applied, as distinguished from Pure Mathematics, it has been accomplished, almost unperceived, under the guidance of men like Stokes, Thomson, Clerk Maxwell, and his successor, Lord Rayleigh; who combine the highest powers of numerical analysis with the imaginative, constructive, and inventive faculty of Wheatstone and Faraday.

At the sister University of Oxford the case is very different. Here the method of the schoolmen and the misrepresented teaching of Aristotle reigned supreme until our own time. The anachronism was indeed expressed in concrete form by a single word. The 'Science,' which up to 1852 formed one foot of the tripod, with Scholarship and History, on which Honours were adjudged, was the Science of a thousand years before, the Metaphysics and Moral Philosophy of the Stoics,—of those who, proposing to teach it, wrote over the entrance to their school, οὔδεις ἀγεομετρήτος εἴσιτο, which, in the terms we are now using may fairly be translated, 'Let none unacquainted with Physics enter.' It was a purely mental analysis of the great problems even then seen to underlie our simplest conceptions of the universe. The change required in this centre of learning was therefore from Metaphysics to Physics; it was a scientific putting of the cart before the horse; a substitution of Pythagoras or Archimedes for Plato or Aristotle, as the latter then

and there was studied; namely, in his dogmatic treatises on Ethics, Politics, Rhetoric, and Metaphysics, and not in his far stronger genius as a natural historian and zoologist.

Is it to be wondered that the wrench thus suddenly given produced molecular change? that the impulse overran the neutral point? and that those who previously had been commended for accurate knowledge of the metaphysical attributes of God, should require time to learn the internal economy of a Holothurian, the exact chemical constitution of ethylic-diethyloxamate, or the formula for Carnot's reversible Heat-engine? Even now, within an ace of thirty years from this intellectual cataclysm, poor old Oxford is only just recovering from a protracted state of vertigo, and settling down again to useful work. It is sad that she should have to chronicle the early loss of one who has been a main agent in the revolution. The Linacre professor of Physiology, who began as an orthodox First-classman in the school of *Litteræ Humaniores* in 1850, dies in 1881 at the age of fifty-two an advanced exponent of modern views in Anthropology.

In the modern University of London, instituted forty years ago, we see another phase of the phenomenon which has been ably and convincingly commented upon by Dr. Bristowe, in a pamphlet confidentially communicated at first to Earl Granville, the Chancellor, but now *publici juris*. His object is mainly connected with the special study of Medicine, and the sustentation of the profession to which he and the present writer specially belong. But he makes incidentally so many shrewd and convincing comments on science-teaching and on its general tendencies, that they fully deserve reproduction. He commences by observing that for some reason or other the University of London has hitherto failed to take that position in relation to the medical profession which the founders of the University doubtless expected of it, and which it is important it should hold. He shows that in forty years only 761 candidates have graduated in medicine, or an annual average of nineteen, only representing a single man for each of the medical schools in England. He compares this poor result with those of the Universities of Paris, Vienna, Berlin, and Edinburgh, even with those of Glasgow, of Aberdeen, and of the Queen's University in Ireland. After commenting on several possible causes of this comparative failure, he continues:—

'But is it not possible that the examinations are too exacting, and so tend to starve the University and do injustice to the candidates? And is it certain either that the medical graduates owe their intellectual eminence to the ordeal to which they have been subjected, or that they have any such collective pre-eminence as justifies the plea for maintaining the present exclusiveness of their body?

'In reference to the former question, having first directed your Lordship's attention to the fact that only a small minority of the students entering the medical profession in England offer themselves as candidates at the Matriculation Examination, let me trace the subsequent history of this minority. Nearly fifty per cent of the total number fail at Matriculation; of those who, having matriculated, present themselves at the Preliminary Scientific, again nearly fifty per cent fail; of those who, having passed the Preliminary Scientific, present themselves at the first M.B. Examination, nearly thirty-five per cent fail; and of those who finally become candidates for the degree of M.B., nineteen per cent are rejected. The collective result is (allowing for the fact that no inconsiderable number of the candidates faint by the way, and never proceed beyond the Preliminary Scientific or First M.B. Examination) that not ten per cent of the young men who enter at the lowest of the series of examinations emerge successful from the last; and that at least seventy-two per cent of the whole number are rejected at the Matriculation and Preliminary Scientific Examinations—examinations in subjects which, with scarcely an exception, have no direct bearing on medicine. Surely the fact that the examinations required of aspirants for the M.B. Degree are so devised and so conducted, that they involve the rejection of nearly three-fourths of them before they have been afforded the slightest opportunity of displaying their acquaintance with any of those branches of knowledge which are the almost exclusive objects of their study in the Medical Schools, which alone it is essential that a medical man should know, and which ought to be the real basis of a Degree in medicine, is sufficient to condemn them as tests of fitness or unfitness for the medical membership of the University. As a matter of fact, those who have never presented themselves at the portals of the University, together with those who have lost heart by being rejected at its earlier examinations, include the great bulk of the more distinguished pupils at our hospitals; of whom many leave us before the completion of their pupilage to gain in Scotland, Ireland, or elsewhere, the Degrees practically denied them in London.

'But the chief cause is the difficulty of the preliminary examinations. This has always been excessive, but of late has been scandalous. The difficulty and indeed the uncertainty of the results are now so fully recognized, that many students who under other conditions would have become Candidates, feeling the importance of utilizing the time at their disposal to the best advantage, do not think it worth their while, or do not dare to face them. Again I appeal to statistics. In the twenty-three years ending with 1860, the rejections at the Matriculation Examination were under 16 per cent. In the next eighteen years, without any apparent cause, they rose to 49 per cent, or to more than thrice their former average; and during the last three years of this time, they actually exceeded 50 per cent. It was in the year 1861, from about which time the extraordinary increase in the rejections at Matriculation dates, that the Preliminary Scientific Examination was instituted, partly to relieve the first M.B. of some of its less strictly medical subjects, partly to insure a more exact knowledge of those subjects on the part of the student. A new obstacle was thus interposed in the medical student's career, an obstacle so serious that it has involved from first to last the rejection of 47 Candidates out of every 100.

It might at least have been supposed that the increasing stringency of the Matriculation Examination, and the further filtration of Candidates through the Preliminary Scientific, would have had the effect of insuring better results in the First and Second M.B. Examinations than had been previously attained. Not in the least. For whereas down to 1860 the rejections at the First M.B. were only 27 per cent, and those at the Second M.B. only 16; since 1860 the rejections at the First and Second M.B. have been respectively 35 and 19 per cent. Collecting these several results, it appears that whereas prior to 1861 the rejections at the *three examinations* which medical graduates had then to undergo, were severally 16, 27, and 16 per cent; from 1861 inclusive the rejections at the *four examinations* now in force have been severally 49, 47, 35, and 19 per cent; and that while in the former period the chances were that out of every 100 medical Candidates at the Matriculation Examination, 51 would become Bachelors of Medicine, in the latter period the chances of final success have dwindled down to 14 per cent.

'The fault of the regulations of the University in reference to the Matriculation and Preliminary Scientific Examinations, is that examination in too wide a range of subjects, and too extensive a knowledge of them, are required of Candidates who, for the most part, are fresh from school, or at any rate have had very little time for serious study after leaving school. The mere enumeration of the subjects required of a Candidate at the Matriculation Examination, is sufficient to condemn it as an entrance examination. They are as follows:- Latin; two out of the three languages, Greek, French, and German; English; English History with Modern Geography; Arithmetic; Algebra, up to and including simple equations; the first four books of Euclid; Natural Philosophy, including Mechanics, Hydrostatics, Hydraulics, and Pneumatics, Optics and Heat; and Chemistry. Why, even a public school-boy of high attainments could not pass such an examination in virtue only of the knowledge he has been acquiring for many years at school; and in the case of boys of average intelligence and education, it involves an outrageously severe and altogether unremunerative mental and physical strain. Who among the Senate even, or who among the examiners, is competent at this moment to pass successfully through such an ordeal? The Preliminary Scientific Examination, again, is one of exceeding difficulty, far too difficult, indeed, for that period of a student's career at which he is required to pass it. Moreover, as I have already observed, it embraces a list of subjects, scarcely one of which has any direct bearing on medicine. In fact, they are placed here in order to get rid of them at once and for ever, so that they may not embarrass the student's further progress. The subjects are:—Mechanical and Natural Philosophy, treated far too profoundly for most men who aim at a degree in medicine; Zoology, which can only be satisfactorily studied after the student has learnt anatomy and physiology; Botany, which is of very little, if any, use to a medical man; and Inorganic Chemistry. Is it astonishing that a large number of youths fail to pass the examination?

'The faults in the conduct of the examinations relate to the quality of the questions which are set, and to the standard of merit which the examiners adopt. As to the general quality of the questions, I have looked through paper after paper set at the Matriculation and Preliminary Scientific Ex-

aminations, and have been struck over and over again with the relative abstruseness of the questions, their inappropriateness or their unfitness as tests of a Candidate's actual knowledge. And I feel sure that most competent and unbiassed persons, who will take the same trouble that I have done, will acquiesce in the justice of my criticism. The question as to whether the examiners, generally, set up too high a standard of merit is one of great importance, yet one which I can scarcely venture to answer decisively. There are several considerations, however, which convince me that my opinion on the matter is in the main correct. They are, the gradually increasing ratio of rejections to passes; the general character of the questions proposed; and the fact, to which I can myself testify, that (apart from the personal qualities of the examiners) the traditions of the University favour excessive strictness of examination, and the maintenance of a very high standard of merit. I may remark in reference to this subject, that young and inexperienced examiners tend to be needlessly and unjustly severe; and that examiners in subjects of relatively little importance are apt to assert by undue strictness of examination the importance of their specialities.

'Finally, there are two other points in relation to the examinations which might, I think, be reconsidered with advantage both to the Candidates and to the University. *First*, I have never yet been able to understand why, when a Candidate rejected at any examination has taken a first class in one or more of the subjects of that examination, these successes should not be counted to his credit when he offers himself a second time for examination. It would be a great relief to the examiners, and not only a great relief but a simple matter of justice to the Candidates if such a practice were adopted. Of the extreme hardship, if not unfairness, of the present rule I can quote conspicuous examples. *Second*, I have also never yet been able to understand why the Senate should forbid absolutely that men who, at a comparatively late period of their studentship, or even at some subsequent period of life, desire to become graduates of the University, and who are able to adduce satisfactory evidence that they have complied strictly with all its essential requirements, should be allowed to present themselves for the successive examinations at the shortest possible intervals. Surely, under such circumstances, the only proof of fitness for its Degree the University can reasonably require is competence to pass its examinations. I have no wish for the University to relax its regulations so far as they affect the generality of students; but I am sure it would be well for it to reserve to itself the liberty of dispensing with some of its merely formal requirements in exceptional cases. The want of such a power, or the refusal to exercise it, has lost to the University many graduates who would have done it honour.

'It has not been under the guidance of such a spirit as this, that, with all their faults, the Universities of Oxford, Cambridge, and Dublin, and the great Continental Universities, have gained reputation and success: it is not thus that the University of Edinburgh has acquired and deserved its world-wide fame as a Medical University; nor is it thus that the University of London will ever attain that position in relation to its affiliated Medical Schools which the University of Edinburgh holds to the Medical Schools of Edinburgh.'

The great vigour and dispassionateness which characterize the criticisms of a leading scientific physician, himself a distinguished graduate and prizeman of the University of London, is abundant excuse for the free citation which the writer has indulged in. The argument here given is moreover substantiated by statistical evidence which is beyond comment. It is really a pressing question, for of all wild beasts which are liable to be let loose on an innocent and unsuspecting public, perhaps the most dangerous is the school-master *in excelsis*. Against him there is often no court of appeal, and no living man except the licensed keeper of lunatics is more liable to intellectual contagion. Every sensible man is cognizant of the pedantry and harshness which often characterizes the proceedings of the School-board Inspectors. It were well if public opinion, the only higher authority, looked more closely after the higher and now practically irresponsible leaders of our studious rising generation. *Quis custodiet ipsos custodes?* is a motto which applies not only to the blue-coated guardian of the public peace, but even more to the established regulators of our national education.

REVIEWS.

THE CAT.*

IT was a happy conception on the part of Professor Mivart to teach natural history on the basis of careful study of a single type in each class of animals. In this he follows out the precept and example of Prof. Huxley, but in a way that is entirely his own. The Cat appears to have been selected for study because, from the ease with which examples may be obtained, it is better suited for examination than any other animal, while many might anatomize the Cat who could have no opportunity of dissecting the human subject. In construction, the plan of the work is novel, in so far as the author presupposes no previous knowledge on the part of his reader, but provides him with the elementary physiology and anatomy necessary to appreciate the subjects discussed; so that in parts the book has a curious appearance of mixing knowledge at once elementary and abstruse, which may disappoint the naturalist who anticipates an exhaustive monograph on the Cat, but is helpful to all beginners. The work is well written, well illustrated, clearly arranged, and is certain to exercise a great influence on the future study of biology, because this is the first time in which so many modes of regarding an animal have been presented simultaneously, and on the whole with such freedom from technicalities. Remembering that it is a book for students, it must be pronounced well done.

The volume opens with a skilfully arranged introductory chapter, giving a zoological and historical account of the different domestic cats, and then passes on to define the several natural-history studies to which the Cat and its kindred may lead. Thus gently leading the reader, the author proceeds to consider the cat's form and skin, or, as he prefers to term it, the external skeleton. Here we begin to make acquaintance with the elementary tissues connected with the skin and skeleton, which are excellently illustrated by well-drawn woodcuts, some of them devoted to illustrations of the teeth of the cat and tooth structure. At the third chapter the skeleton is reached. The vertebræ are fully described and well figured; the skull is excellently drawn, and every bone figured in detail, showing internal and external aspects exactly as though the treatise concerned itself with the

* *The Cat. An Introduction to the Study of Back-boned Animals, especially Mammals.* By St. George Mivart, Ph.D., F.R.S. With 200 illustrations. 8vo. London: John Murray. 1881.

anatomy of man; though not being written exclusively for a learned class of readers, the style is somewhat less technical than is usual in anatomical works. The osteology of the limbs is similarly treated, sometimes with admirable drawings of the ligaments; but this part of the work might without doubt have been improved, had some further attention been given to indicating the attachments of the muscles to the bones. The fifth chapter describes the cat's muscles, leading up to a consideration of their arrangement by a few pages on muscle structure. Here there seems to be some deficiency of illustration; for though the student trained in the usual way would have no difficulty, further aids in dissection might be required by the reader for whom the book is designed. The chapter on the cat's alimentary system is followed by a discussion of the circulatory organs. Then succeed chapters on respiration and secretion. The ninth chapter deals with the nervous system and organs of sense, and this is in some respects the most carefully prepared chapter. The tenth chapter treats of the development of the cat, and of the subject of development generally. This is perhaps the weakest chapter, since there is comparatively little said which might not as well have occurred in a treatise on any other mammal.

The anatomy here ends. It is throughout plain, descriptive exposition; nothing whatever is said, such as might have been anticipated from the introductory chapter, of the comparative anatomy of the cat. There are no indications of the modifications which the skeleton assumes in other carnivora, and no indications of the distinctive elements which define the carnivorous skeleton of which the Cat is the type. This same want of grasp extends to the study of the muscles and all the other organs; yet the fact that the Cat is taken, as the title tells us, as 'an introduction to the study of back-boned animals, especially mammals,' would have led us to anticipate that, after studying the type, we should have been led up to a contemplation of the ways in which it is modified. This is a want of symmetry, and we cannot but think that such knowledge would have vastly augmented the student's interest in his work, and have paved the way for his contemplation of the higher problems of anatomy and physiology which must inevitably be forced upon his attention. It is in no way met by the chapter on the Cat's place in nature.

The author entitles the eleventh chapter the Psychology of the Cat; but here we find a great deal about psychological questions, and less than we could wish about the Cat. It is true, as the author remarks, that we cannot, without becoming cats, perfectly understand the cat mind, and this may account for the difficulty obviously experienced in expounding the subject. When he comes to the analysis of mental powers which the Cat is stated not to possess, there is sure to be difference of opinion among his readers, in proportion to their observation from nature and psychological training; for he denies to cats all the truly rational faculties, and would draw a sharp line between the mind of man and mind in brutes. The soul comes in for consideration, perhaps for the first time in a work of this character; but the author uses the term in a sense somewhat different from that usually accepted. With him it is 'that principle of individuation which makes the animal what it is, though it has no actual existence apart from the matter it vivifies; yet it is the animal *par excellence*, the matter

which it is composed being but the subordinate part of that compound but indissoluble unity—the living animal.' The twelfth chapter, entitled the Different Kinds of Cats, describes in brief zoological fashion, fifty species of the Cat kindred, with the aid of illustrations. Then succeeds some account of fossil cats, or Felidæ, in which are represented a number of new genera defined by Professor Cope. Here, again, we miss the analytical conclusion which ought to have followed, pointing out the relations of cats to each other, and the differences which distinguish the animals mentioned from each other. The thirteenth chapter is an article on the Cat's Place in Nature. This is really a statement of Prof. Mivart's views on classification; and after the lower animals have been disposed of, the Cat is ingeniously compared with the lower Vertebrates merely to exemplify their structure; and then the distinction of Carnivora from the other groups of mammals is given. Some detail is gone into concerning points in the base of the skull which distinguish bears, dogs, civets, paradoxures, hyænas, and other animals, and the distinction of the Felidæ from each of the allied families is formulated in a series of characters drawn partly from external structure and partly from anatomy.

The Cat is considered to be the typical genus of its family; the cheetah is a generalized normal form; the lion a specialized normal form; the Canidæ are said to be the most generalized aberrant form; bears depart from the normal standard of the order in their own way. But the author's views incline him to state that 'something may be said in favour of cats being the highest of mammals, the very flower and culmination of the mammalian tree.' This really looks like a sop in the pan for the Cat, after it has been anatomized.

The last chapter but one is the Cat's Hexicology, which is explained to be its environment, or relations to space, time, physical forces, and conditions of life. First it is observed that no cat dwells with the polar bear, and that for some species no region is too hot. Some species are diurnal; none are aquatic; and they drink but little water. An interesting but brief account is given of the geographical distribution of the Felidæ; but there could be no good reason for introducing an account of the recognized natural history regions of the earth with their characteristic animals. Then follows a short account of the palæontological history of cats. The oldest are found in India and Greece, in beds of older Pliocene or newer Miocene age; and the chapter concludes with some account of the various internal and external parasites with which cats are troubled. We are sure that most readers will be comforted to learn that Hexicology is a science; but how parasites, extinct allies of the tribe, geographical distribution, and the influence of phenomena of nature, can hang together in scientific relation, is not much clearer after Prof. Mivart's exposition than it was before.

The volume closes with an account of the pedigree and origin of the Cat. It is contended that the cats were derived from the Insectivora, which the author would take as the parent stock of all Mammalia, instead of deriving the higher forms from the Marsupials; and he argues that *Hyænodon* and other tertiary mammals supposed to have marsupial affinities, are just as likely to be related to the Insectivora. The views adopted would lead to the belief that the old mammals of the Stonesfield slate are also insectivorous, against which

something might be said. An excellent tabular scheme gives the conclusions at which the author arrives as to the pedigree and affinities of the animals with which the cat is most closely related; and then, as was to be expected, the author passes on to consider the Cat's origin, or, in other words, the doctrine of evolution in those aspects in which it commends itself to Prof. Mivart; his contention now, as ever, being for an Individual Divine government of the universe, while the mass of scientific men are seeking to discover what the government really is, and how it operates for the well-being of the world.

The book is strongly tinged throughout with the author's individuality; and it is obviously the outcome of his best and most earnest labour for the advancement of sound learning.

MUSCLES AND NERVES.*

THIS is one of the volumes of the International Scientific Series, and was written by the learned and intelligent Professor in 1877. He dedicates the book to the illustrious Emil Du Bois-Raymond, his master, and writes, in the Preface, that 'this attempt at a connected account of the general physiology of muscles and nerves is, as far as I know, the first of its kind.' He makes the following extraordinary assertion:—'But anyone who endeavours to gain an idea of this branch of knowledge from the existing text-books of physiology will probably labour in vain.' Recalling, therefore, what we learned from Todd and Bownan, who had the advantage of witnessing the experiments of Mateucci, and who were perfectly acquainted with the work of Faraday on *Electrical and Galvanic Currents*, it is interesting to make out how much further Dr. Rosenthal, the careful recorder of the work of Ed. Weber, E. du Bois-Raymond, and H. Helmholtz, carries us.

The first chapter relates to molecular and protoplasmic motions and ciliary motions, and there is no advance in it; and in the second chapter there are the good old truths regarding muscles, their form and structure. But then a new subject is opened up, and the muscle begins to be treated as a lifeless thing, and the physics of elasticity are explained. Some very simple facts are proved over again by physical experiment—the principal use of physiological laboratories—and the conclusion come to is—'The muscle on contracting is capable of lifting a weight. The same weight, however, extends the muscle, and the co-operation of the two forces—the contractile tendency and the elastic extension—produces, as we shall find, the final operation on which labour depends.' There is nothing new in this, except the new 'force' of contractile tendency, which really means contraction pure and simple. The author deals with tetanus, and thinks that he is bringing us nearer the comprehension of that muscular condition by com-

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* *General Physiology of Muscles and Nerves.* By Dr. S. Rosenthal, Prof. of Physiology in the University of Erlangen. 8vo. London: C. Kegan Paul & Co. 1881.

paring it with the enduring contraction which is the result of the action of rapid alternate currents of electricity. Certainly the phenomena are analogous, but by no means identical. The author states that warmth and internal motion exist in the muscles during tetanus, but this is not new, for ruptured fibres have been seen under the microscope for the last forty years. Then we are told that the absolute force in the human muscle is equal to six to eight kilogrammes. We presume that the author means energy. But there is a struggle between common sense and physics in the calculation. Weber proved by experiment that the whole force (energy?) of the calf muscles of man, of both legs, 'is greater than the weight of the body.' How can a child raise itself on its toes and jump up in the air if this is not true?

The author states that it is easy to show that chemical processes occur within the muscles, but that our knowledge of the chemical constituents is very incomplete. He explains Helmholtz's experiments to prove that muscular contraction produces warmth,—rather a work of supererogation. The rigor mortis is put down to the coagulation of some liquid in the living muscle and to the formation of an acid. The book is full of illustrations of different methods of demonstrating the relations of electricity and muscular action, and it is to be feared that if it is carefully studied people will not care much about having to take the useless trouble of explaining vital phenomena by those of pure physics.

THE BRAIN.*

THIS is a book which ought to be in every library, for it is full of good facts and solid physiology, and contains many interesting hypotheses. It treats first of the nervous systems of the Invertebrata, and considers the data concerning the human brain which are derived from the study of them; and then the brains of fishes, amphibia, reptilia, and birds, are considered in relation to reflex action, sensation, ideation, and perception. Instinct is considered nascent reason also. After dealing with the nervous centres of the higher Vertebrates, Dr. Bastian considers their mental capacities and the relation of brute and human intelligence. There is a capital chapter on 'Phrenology Old and New,' and the book ends with a discussion of the problems relating to the localization of higher cerebral functions. It is a well-written work, and worthy of the reputation of the author.

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* *The Brain as an Organ of Mind.* By H. Charlton Bastian, M.A., M.D., F.R.S. 184 Illustrations, pp. 708. The International Science Series, Vol. XXIX. 8vo. London: C. Kegan Paul & Co. 1880.

THE SPIRIT OF NATURE.*

MR. Baildon writes like an accomplished gentleman; he has plenty of ideas, a command of good English, and does not fear, even in these days of practical Philistines, to elaborate elegant and eloquent sentences. He has studied the writings of the advanced school of evolutionists, and decides that physical and physiological properties are absolutely independent of moral conditions. He states, 'Something of the history of organisms, of physical evolution, we are beginning to know, but spiritual evolution is still a mystery.' In these ideas he will be supported by many more evolutionists than he thinks. There is an interesting chapter in which the author deals with science and poetry, and he comes to the conclusion that science is no more antagonistic to poetry than it is to religion. He follows it up by a good attack on the doctrines of Automatism and on the conclusion-jumping of the advanced school. But earlier in the volume Mr. Baildon mistakes Charles Darwin's views, and in fact he states, 'I have read a great part of Mr. Darwin's *Origin of Species*.' He might have studied the whole over and over again, and with benefit, and if he had he would not have written the following nonsense:—'His followers have seen the weakness of his position, and have many of them gone over to atheism. Now I trust I shall not be misunderstood, but that you will bear with me till I have fully explained myself, when I say that science must always be in a sense atheistic. By atheistic here is meant not what denies Deity, but what leaves it out of account. Science, so far as it seeks only the particular finite, intelligible or secondary cause, and has no concern with the universal, infinite or first cause. When, for example, we say to a child that God made such and such a thing we give the child no scientific information whatsoever, though we impart a religious truth.'

Cruelty of Nature is the heading of one chapter, and is eminently polemical and of course personal. He calls the Laureate a Christian calumniator of nature, and opposes the idea that a charge of cruelty can be established against nature. But the conclusion he comes to is that nature is not guilty if the pain and misery of men and animals is of any use. It must be shown that the pain occasioned by her processes is unnecessary or unjust. He jumps at the conclusion that the necessity of pain has been established because of the existence of pleasure, and the use of pain is a warning. We cordially recommend the book to sober-minded readers of all shades of opinion, and believe that they will come to the conclusion that evolution and science are compatible with the belief in a Creator whom man may approach through his intellect.

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* *The Spirit of Nature, being a Series of Interpretive Essays on the History of Matter from the Atom to the Flower.* By H. B. Baildon, B.A. Cantab. 8vo. Pp. 216. London: J. & A. Churchill. 1880.

ANTHROPOLOGY.*

MR. E. B. TYLOR'S Manual of Anthropology does not profess to be an exhaustive treatise upon the subject, and indeed, as he himself says in his preface, some portions of the science of man are designedly handled in a somewhat superficial fashion. This statement relates more especially to the ethnographical section, in which the author has avoided detailed reference to the more technical treatment of the physical characters of the various races of mankind. The ordinary student, however, will find quite sufficient information in the first three chapters, which deal with what may be called the natural-history side of the subject, to satisfy his requirements; those who wish to study this branch more profoundly will easily find no lack of other guides.

The remaining chapters are devoted to an exposition of the principles of the science of Anthropology. Taking, as his starting-point the notion of the primitive barbarism of our earliest ancestors, Mr. Tylor shows in a very clear and interesting manner the mode in which the different characteristics of man as a social creature have originated and attained to their varying degrees of development; and by wide, but judicious generalizations, and a most careful treatment of the matters in hand, he has certainly succeeded in producing a picture of the course by which mankind have attained to their present state which leaves little to be desired.

After dealing, as already stated, with the general natural-history facts of Anthropology, Mr. Tylor passes in review the development of the special characteristics of man, naturally commencing with language, as being the distinction between man and the lower animals upon which the progress of the human race in all other respects mainly depends. The art of writing of course constitutes a sort of appendage to language. The four following chapters relating to the arts of life and dealing with the invention and improvement of weapons and instruments of all kinds, leading up to the most complete machinery at present in use; the progress of mankind in the matters of dwellings, clothing, and navigation; the use of fire and cookery, which used to be cited as the one recognizable distinction of man; the manufacture of vessels of various kinds, and the employment of metals in the arts and as money, will be read with the greatest interest, as they contain an infinity of curious information brought together from the most various sources, and open up many new views of the connexion between old and frequently obsolete practices, and those of the most advanced civilization.

One chapter is devoted to the arts of pleasure, under which head Mr. Tylor includes, among other things, poetry and music, dancing, the drama, the imitative arts of sculpture and painting, and games. The remaining chapters deal with the origin and progress of scientific knowledge, of spiritual ideas and religion, of history and mythology, and of society.

As we have already stated, the book is most interesting and suggestive, and so far as we are able to judge, the materials have been selected with

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* *Anthropology: an Introduction to the Study of Man and Civilization.* By Edmund B. Tylor. 8vo. London: Macmillan and Co. 1881.

great judgment, and most carefully placed in the clearest point of view. Here and there, indeed, we detect small errors,—for instance, in speaking of costume the author seems to misinterpret the significance of the two useless buttons on the backs of our modern coats; and in another place he represents the French future tense of verbs as formed by adding the present tense of the verb *avoir* to the infinitive of the verb, so that *je donnerai* would signify, 'I have to give'! an absurdity which we are sorry to find in so good a book. The illustrations are good and tolerably numerous, especially in the natural-history chapters.

MONTICULIPORA.*

A YEAR and a half ago we had occasion to notice an important work on the so-called Tabulate Corals, then lately published by Prof. H. Alleyne Nicholson. In this book Prof. Nicholson referred, in immediate connexion with the Chætetidæ, to a group of organisms forming the genus *Monticulipora* and some allied genera or sub-genera, and he has now produced an equally elaborate treatise, which is devoted exclusively to those curious and problematical fossils.

The genus *Monticulipora* was founded by D'Orbigny in 1850, for the reception of certain Palæozoic corals, which he defined as having the cells close together, poriform, at the surface of a branched or incrusting whole covered with small conical projections; and it was from the presence of these little elevations or 'monticules' that the name of the genus was derived. Other forms showing more or less agreement with the characters laid down by D'Orbigny were soon found in various parts of the world, or previously described species were referred to the new genus, whilst in some cases new genera were formed for the reception of new forms; but the whole were still always considered to belong to the Corals, and in fact to the section Tabulata, after the establishment of that group by Milne Edwards and Haime.

Of late years, as all palæontologists know, the rights of the group of Tabulate corals to exist as a systematic entity has been frequently called in question, and we may hold that the *coup-de-grace* was given to it by Prof. Nicholson in the work already referred to; but, as will indeed be seen even from our imperfect notice of that book, the business of negation was easier than tha of affirmation, and while it might without much difficulty be demonstrated that the fossil organisms in question do not form a systematic whole, it was by no means an easy task in all cases to decide where the dismembered parts should be placed.

In his former book, Prof. Nicholson was inclined to regard *Chætetes* and *Monticulipora* as Alcyonarian forms, notwithstanding the arguments put forward by Prof. Lindström and others in favour of their being placed

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* *On the Structure and Affinities of the genus Monticulipora and its Sub-genera; with Critical Descriptions of Illustrative Species.* 8vo. Blackwood and Sons, Edinburgh and London, 1881.

with the Bryozoa (or Polyzoa). This question is discussed by him at considerable length in his new treatise, and he still holds, in opposition to the views of Prof. Lindström and Busk, and of Mr. Waters, that there is no unmistakable evidence of the Bryozoan nature of the Monticuliporidæ. With regard to their relationship to the genus *Heteropora* especially, upon which much stress has been laid, he says, 'It is clear that the points of likeness between the two are by no means so weighty as the points of difference. On the one hand, we have a strong natural resemblance, a general similarity in the mode of construction of the skeleton and an agreement in the fact that in both genera the colony consists of two sets of tubes, crossed by transverse partitions. Such transverse partitions of the tubes (or, as we may loosely call them, "tabulæ") occur, however, in organisms of such exceedingly diverse affinities, that we can, admittedly, attach no value to the last-mentioned of the above resemblances. . . . On the other hand, to set against the mostly superficial points of resemblance above noted, we have a number of fundamental structural differences. Thus, in *Monticulipora* the walls of the tubes are imperforate; there are no traces of radiating spines or "septa," and in the dimorphic or trimorphic species there are usually important differences as regards the different groups of corallites. In *Heteropora*, on the contrary, the walls of the tubes are traversed by a very remarkable and exceptionally developed canal system; the tubes possess in their outer portions a well-developed series of radiating spines . . . and the interstitial tubes are in no way structurally different from the proper zoœcia.' From these and other considerations, the author declares, without denying the possibility that *Monticulipora* may belong to the Bryozoa, that his specimens lean decidedly towards the Cœlenterata 'as a proper resting-place for the genus.'

In the systematic and descriptive part of his present work, Prof. Nicholson makes no attempt at a monograph of the curious and puzzling group of fossils to which he has devoted so much attention, but simply endeavours to define clearly and accurately the species with which he is acquainted, and the generic and sub-generic groups to which they are best referred. His family Monticuliporidæ is made to include only the single genus from which it takes its name; and the other named groups of allied organisms, some of which were distinguished even before D'Orbigny established his genus, are treated provisionally as sub-genera. Of these there are *Fistulipora*, M'Coy, with which *Callopora*, Hall, is synonymous; *Constellaria*, Dana, including *Stellipora*, Hall, and *Dekayia*, M. Edw. and H.; and these three groups are regarded by the author as so far differing from the rest that their distinction may be considered of more weight, and they thus acquire a sort of quasi-generic value, in opposition to *Monticulipora* proper, which in its turn includes the subordinate groups, *Diplotrypa*, *Heterotrypa*, *Monotrypa*, *Prasopora*, and *Peronopora*—the last-named here proposed for the first time.

It would be manifestly out of place here to enter upon a criticism of the course adopted by Prof. Nicholson, if, indeed, anything of the kind could be profitably undertaken without a long study of the very objects investigated by him. The reader will easily see, from what is stated above, that the subject is an exceedingly difficult one, and that the attempt at

classification leads to a sufficiently complex result—evidently, as the author justly says, the time has not yet come for a final monograph of the group. But in his characters of the genera, or sub-genera, and his long and detailed descriptions of the structure of numerous so-called species belonging to the sub-genera of *Monticulipora* proper, Prof. Nicholson has certainly done important work towards the perfect elucidation of these most difficult fossil organisms, and the abundance of admirable figures that he has given, both on half-a-dozen plates and as woodcuts scattered through the text, add immensely to the value of his work. His opinions as to the nature of the organisms are still *sub judice*, but whether they are right or wrong, his contribution to the history of the Monticuliporidæ will certainly prove a great help to future investigators.

SYSTEMATIC MINERALOGY.*

IN describing the various crystalline forms, the author of this volume deals only with their symmetry and general geometrical proportion, without entering into the question of the practical calculation and determination of individual examples. In doing this, the methods followed are mainly those of Groth's treatise on 'Physical Crystallography,' excepting that the plan there adopted of considering the physical structure of crystals before their geometrical properties has been abandoned in favour of the more common one of giving precedence to the latter.

The optical properties of crystals are considered at somewhat greater length than is usual in rudimentary books, and the phenomena of polarization receive special notice. A mixed system of crystallographic notation has been adopted. In the text, the forms are designated by their symbols according to Naumann, while the notation of their forces is by indices on Miller's system. It is explained that this has been done with a view of familiarizing the students with both systems, since, although the notation of Miller is probably the best, that of Naumann is more generally employed both in text-books and in original memoirs. The classification followed in the chemical portion of the volume is that of the second edition of Rammelsberg's *Handbuch der Mineral-Chemie*.

This book is not only a safe and useful guide to students desirous of acquiring a general knowledge of mineralogy, but will also serve as an excellent introduction to the large text-books of Dana, Miller, Descloizeaux, and Schrauf.

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* *Text-book of Systematic Mineralogy.* By Hilary Bauerman, F.G.S., Associate of the Royal School of Mines. London: Longmans, Green, & Co. 1881.

SIGHT.*

THE function of sight has engaged so much study, that one marvels that no one should till now have undertaken to explain its chief phenomena to the reading public. The physiological aspects of the eye have been fully discussed in Michael Foster's *Physiology*, for instance, and the physical considerations are sufficiently explained in most treatises on Optics; but Prof. Le Conte, in a volume of the International Scientific series, presents us with a larger view of this subject than comes under the consideration of a specialist. The author is a master of the art of professional exposition, singularly clear, full of information, and often original in his own views as well as in stating those of others; and, therefore, his work fills a gap in literature in an able way. The volume is divided into three parts, which concern monocular vision, binocular vision, and disputed points in binocular vision. After a short introduction, which explains the relations of the senses to the phenomena with which they are concerned, the first part gives an admirable account of the elementary physiology of the eye, in four chapters which concern its general structure, the formation by it of images, its properties as an optical instrument when compared with the work of the optician, some of its defects, such as old-sightedness and near and long sight, and the structure and functions of the retina. The second part of the work contains much that is novel, for few persons consider the relation of their eyes to each other. It is shown that everyone sees double, and that most persons are right-eyed, just as they are right-handed, because they are, as the author phrases it, left-brained. And an explanation is given of the reason why objects beyond or nearer than the point of sight are always seen double. The superposition of images, binocular perspective, and judgment of distance, size, and form, are the other subjects of this part. The perception of double images is a distinctive view, and leads the author to certain differences from the theories of Wheatstone and Brücke. The third part deals first with the laws of the motions of the eyes, which are considered to be parallel or convergent motions. A chapter is devoted to the horopter. It is obvious, that as each eye forms its image, and as the images are brought together and superimposed at the point of sight, the sum of the points which appear single, while the point of sight is unaltered, or horopter, may be a line or a plane. The author considers it to be a line. This and many other original views, well set forth, will ensure careful consideration for the physiological studies of one who has already enriched geological science with many new facts and important theoretical views.

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* *Sight: an Exposition of the Principles of Monocular and Binocular Vision.* By Joseph Le Conte, LL.D., Prof. of Geology and Natural History in the University of California. With Numerous Illustrations. 8vo. London: C. Kegan Paul & Co. 1881.

A GEOLOGIST'S NOTE-BOOK.*

MR. John Edward Lee, a well-known geologist and archæologist, and the translator of Keller's admirable work on *Lake-dwellings*, has published the contents of his note-books on geological subjects under the title of *Note-book of an Amateur Geologist*. As he commenced the study of geology under the auspices of Prof. Phillips at a very early period of the latter's career, it will be easily understood that the notes that he has accumulated, and here offers to the public in an unvarnished state, extend over a very considerable number of years; and as Mr. Lee has been in the habit of travelling over the continent of Europe as well as in his own country, his observations refer to an exceedingly wide range of surface. The earliest notes of all relate to the Isle of Wight, and date from 1829, in which year the author also visited France, as appears from notes and sketches of geological phenomena both at Boulogne and in the neighbourhood of Paris. His residence at Hull at this time, which no doubt brought him into contact with John Phillips, next led to his devoting some attention to parts of Lincolnshire and Yorkshire, and in 1840 we find him making a trip to the Baltic. Subsequently the mountain-limestone district of Yorkshire, the Western Islands and the Giant's Causeway, the Eifel and other parts of Germany, Switzerland, and Italy, were visited by him, whilst others of the notes attest geological activity in many parts of the United Kingdom.

Wherever he went, however, Mr. Lee seems to have kept his eyes open, and to have noted both with pen and pencil whatever struck him as remarkable in the geology of the localities he visited. As might be expected also from the disciple and intimate friend of John Phillips, his geological tastes by no means flowed into a single channel, and we find him taking the same interest in bits of stratigraphical geology, examples of contortion and upheaval of strata, glacial phenomena, traps, basalts, and other igneous rocks, intrusive and otherwise, matters of surface geology and even palæontology, so that the guest at his table has a tolerably various repast spread before him. As a man of taste as well as a geologist, Mr. Lee seems to have paid particular attention to cases in which geological structure of one kind or another has a marked influence upon scenery, and many of his best sketches have for their object the illustration of such points.

Mr. Lee has with justice interpreted his *Note-book* a little widely, that is to say, he has included in it reprints of a few papers, chiefly on palæontological subjects, that he has from time to time published. In this way we get papers on the Sponges of the Yorkshire Chalk, on Saurian Remains from the Wealden, on the Microscopic Structure of the Scales of *Lepidotus*, on the Geology of Nettleton Hill in Lincolnshire, on *Cupressocimus*, &c. The book is thus a summing-up of the results of its author's studies in geology during a long life, and naturally its contents are of a sufficiently miscellaneous nature. The drawings and sketches make 209 plates, and the notes, which occupy ninety pages, consist, for the most part, of very brief explanations of

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* *Note-book of an Amateur Geologist.* By John Edward Lee, F.G.S., F.S.A. 8vo. London: Longmans. 1881.

the subjects of the illustrations, many of which, being facsimiles of the pencil memoranda made on the spot in the *Note-book*, are, as a matter of course, rough enough. Sketchy as the work is, however, the geologist will find many interesting details in it, and we think it is a pity that Mr. Lee, instead of arranging his materials in the order of date, did not adopt some sort of rough classification, such as would have served to guide those consulting his Notes more directly to the matters of which they may be in search.

TIN. *

TIN is one of those metals in which an Englishman may naturally take great interest. For many years it was supposed that the tin which furnished ancient nations with the means of making bronze was exclusively derived from the south-western corner of Britain; and although this source of national vanity has been long dried up, there is no doubt that a very large proportion of the tin used in the world until quite recently was obtained from the Cornish mines. Nowadays, indeed, the development of tin-mining in other countries, and especially in the far east, where labour is cheap and the stanniferous deposits are still comparatively untouched, has thrown the old Cornish industry into the background, and it will probably be many years before the British stannaries can attain anything like their former prosperity.

Nevertheless the production of tin, although it may be regarded as one of the minor metallurgical industries when compared with that of iron, copper, or lead, is of great importance, as the metal is applied to many purposes in which it would be hard to find a substitute for it. Those who take an interest in mining matters will therefore owe a debt of gratitude to Dr. E. Reyer, a distinguished Austrian geologist, who has devoted a great amount of labour to the investigation of the occurrence and working of tin ores in various parts of the world, and now publishes the results of his researches in a small octavo volume.

As will be seen from the title of his book, he views his subject from all sides, except that of the actual smelting, although some particulars of the results of this operation are also given. He commences with some Bohemian and German localities,—Zinnwald, Altenburg, Graupen (where tin was mined as early as the year 1200), and Schlackenwald, then deals with Cornwall and some very limited occurrences of tin in the Iberian peninsula, France, Italy, and Finland, and finally describes the stanniferous regions of Malacca, Banka, and Bilitong, and the most recently discovered tin-grounds of Australia and Tasmania. The metal is treated in connexion with these districts under the various aspects of its geological distribution, the mining of the ore, and the history of its production in each locality, the whole forming a compendious treatise which has every appearance of being thoroughly trustworthy.

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* *Zinn, eine geologisch-montanistisch-historische Monografie.* Von E. Reyer. 8vo. Berlin: G. Reimer. 1881.

We may remark, however, that the author adopts that abominable so-called phonetic system of spelling, which, thanks to the good sense of the Government authorities, did not succeed in establishing itself as the state rule of orthography in Germany. It is melancholy to see men of cultivation giving in to a system of spelling which destroys the affiliation of the words in their language, and this, especially in the case of German, with no advantage that can be considered in any way to make up for the loss. Thus, in Italian and Danish, we are accustomed to see *f* used in place of *ph*, but this is no innovation, and why people who are accustomed to the use of the *ph* should discontinue it is rather hard to see. In the present book we find our old friends the Phœnicians transmogrified into 'Fönizier,' a word at which we really have to look twice before understanding what it means. But the letter to which these German phoneticists seem to be most inimical is the letter *h*, not from any cockney-like objection to aspirating it, but because it occurs in many words where it has not the effect of an aspirate. One would think that Germany had found itself to be like the country described by Hood in one of his children's tales, 'a land of Toomeniaitches.' Thus *th* is everywhere converted into *t*, and we get such elegant words as *Teil* and *Tat*, and from the latter we get *Täter*, which has a homely sound; and further, the *h* following a vowel is dropped, so that instead of *Jahr*, *mehr*, *sehr*, *ohne*, *wohl*, and *zahlen*, we get *Jar*, *mer*, *ser*, *one*, *wol*, and *zalen*. With regard to these last words we can only say that the German who would make no difference in pronouncing them when spelt in these different ways, has a very imperfect idea of the pronunciation of his own language; and if there ought to be a difference, what becomes of the phonetic spelling?

SCIENTIFIC SUMMARY.

CHEMISTRY.

Synthesis of Ammonia.—Mr. G. S. Johnson, having observed the formation of traces of ammonia while passing nitrogen gas over freshly hydrogenized copper at a red heat, was induced to make further experiments. He passed a mixture of hydrogen and pure nitrogen over spongy platinum at a low red heat. The hydrogen was produced first and passed through silver nitrate and sulphuric acid, over pumice moistened with sulphuric acid, through Nessler's reagent, and finally over the spongy platinum. The platinum having been heated to redness in the hydrogen and the absence of ammonia proved, nitrogen was evolved from ammonium nitrite and passed through the apparatus with the hydrogen. When the mixed gases reached the hot platinum, ammonia was formed, characterized by its odour, by its rendering the Nessler reagent brown, and red litmus blue, and producing white fumes with hydrochloric acid. 5·9 milligrammes of NH_3 were produced per hour in this way. Objections having been raised to this process, on the ground that the vapour of some ammoniacal salt might be carried through the apparatus by the rapid stream of gas, Mr. Johnson made further experiments. Hydrogen was passed through the apparatus and over the spongy platinum, heating the latter until it gave off no ammonia; the platinum was then allowed to cool, and a current of nitrogen was passed through to displace the hydrogen, with the intention of heating the platinum afterwards in pure nitrogen, so that the ammonia salt, if present, might be dissociated and yield its ammonia. On the admission of the nitrogen to the cold platinum, however, ammonia was produced to the amount of 24 milligrammes in an hour. To avoid all chance of error, the author repeated the experiments with nitrogen obtained from atmospheric air, but without success; nor was any ammonia produced when the nitrogen evolved from nitrite of ammonium was passed over heated copper before being brought in contact with the platinum. Hence it was to be inferred, either that the contact with the copper produced an inactive condition of the nitrogen, or that some nitrogen oxide present in the nitrogen was removed by the hot copper. The nitrogen produced from ammonium nitrite was found to contain some nitric oxide, to remove which it was passed through ferrous sulphate, and then, to test its purity, through a tared tube containing freshly reduced copper heated to redness. When the gas has passed over a sufficient length of the ferrous

sulphate, the tube acquired no increase of weight. This nitrogen, entirely freed from oxide, was now passed over cold spongy platinum, mixed with hydrogen, and 3 milligrammes of ammonia were produced in an hour. But when the mixed gases were passed over heated asbestos before reaching the platinum, no ammonia was produced until the asbestos-tube had cooled considerably. Hence the author concludes that nitrogen, like phosphorus, may exist in two states, active and inactive, the latter produced by exposure to heat. (*Journ. Chem. Soc.*, March 1881, pp. 128 and 130.)

GEOLOGY AND PALÆONTOLOGY.

Classification of Dinosaurs.—Prof. Marsh describes the characters presented, by a nearly complete skeleton of the gigantic Dinosaur, named by him *Brontosaurus excelsus*, and indicates a second species of the same genus (*B. amplus*) of about the same size, and several other forms of the same order from Jurassic deposits in the United States. He remarks that the collection of American Jurassic Dinosaurs in the Museum of Yale College, includes the remains of several hundred individuals, many of them in excellent preservation. The series is so complete, that he thinks it may serve as a basis for a provisional classification of the American Jurassic forms, which may be hereafter extended to include the Dinosaurs from other formations in America, and those of other countries. Of this proposed classification he gives the following outline:—

Order DINOSAURIA. Owen.

1. Sub-order SAUROPODA (Lizard foot). Herbivorous.
   Feet plantigrade, ungulate; five digits in all feet.
   Pubes united in front by cartilage; no post-pubis.
   Precaudal vertebræ hollow; limb bones solid.
   Family *Atlantosauridæ*.
   Genera *Atlantosaurus*, *Apatosaurus*, *Brontosaurus*, *Diplodocus*, and *Morosaurus*.
2. Sub-order STEGOSAURIA (Plated Lizard). Herbivorous.
   Feet plantigrade, ungulate; five digits in all feet.
   Pubes free in front; post-pubis present.
   Vertebræ and limb bones solid.
   Family *Stegosauridæ*.
   Genera *Stegosaurus*.
3. Sub-order ORNITHOPODA (Bird foot). Herbivorous.
   Feet digitigrade; four functional digits in the fore, and three in the hind foot.
   Pubes free in front; post-pubis present.
   Vertebræ solid; limb bones hollow.
   Family *Camptonotidæ*.
   Genera *Camptonotus*, *Diracodon*, *Laosaurus*, and *Nannosaurus*.

4. Sub-order THEROPODA (Beast foot). Carnivorous.
   Feet digitigrade; digits with prehensile claws.
   Pubes coossified in front; post-pubis present.
   Vertebræ more or less cavernous; limb bones hollow.
   Family *Allosauridæ*.
   Genera *Allosaurus*, *Creosaurus*, and *Labiosaurus*.
5. Sub-order HALLOPODA (Leaping foot). Carnivorous?
   Feet digitigrade, unguiculate; three digits in hind foot.
   Metatarsals much elongated; calcaneum much produced backward.
   Two vertebræ in sacrum; limb bones hollow.
   Family *Hallopodidæ*.
   Genus *Hallopus*.

6? Sub-order CŒLURIA (Hollow tail). Carnivorous?
   Family *Cœluridæ*.
   Genus *Cœlurus*.

Two of the above genera (*Diracodon* and *Hallopus*) are described as new in the paper from which this classification is taken.—*Silliman's American Journal*, May, 1881.

Silurian Plant Remains.—Traces of these fossils were first observed in 1875 by Dr. Hicks in some shale, interstratified with rather massive beds of grit, in Pen-y-glog slate quarry, about two miles east of Corwen. Further research has resulted in the discovery of more satisfactory specimens, and in showing that they occur at still lower horizons. The fragments are exceedingly abundant, in some places being so closely matted together as to form a carbonaceous seam over an inch in thickness. Some of the specimens are from half an inch to an inch in diameter, and of various lengths, but always in a broken condition, indicating that they did not grow in the position in which they are now found, but must have drifted here on being washed down by river floods. Thin seams of a very pure anthracite, showing occasionally indications of plant structure, occur also in the slates underlying these beds; and anthracite is frequently found composing the nucleus in some large nodules which are found at several horizons in the slates. Among the specimens found are some spherical bodies, resembling the *Pachytheca* of Sir J. D. Hooker, from the lower bed of the Ludlow series, supposed to be Lycopodiaceous spore-cases; also numerous minute bodies stated by Mr. Carruthers to be united in threes, and to agree with the forms of the microspores of Lycopodiaceæ, both recent and fossil; and some fragments, which may belong to these plants, and others, probably belonging to plants described by Dr. Dawson from the Devonian of Canada under the name of *Psilophyton*. The above testify to the existence of a very rich land-flora at the time. Mixed up with these, however, are numerous carbonaceous fragments of a plant described also by Dr. Dawson from the Devonian of Canada, which he referred to the Coniferæ, but which is, according to Mr. Carruthers, an anomalous form of Alga. The former called it *Prototaxites;* the latter renamed it *Nematophycus*. Numerous microscopical sections, showing the beautiful structure of this interesting plant from the specimens found at Pen-y-glog, have been examined by Mr. Etheridge and Mr. Newton, and their conclusions agree in the main with those of Mr. Carruthers. Mr. Etheridge, however, recognizes here a new

species, which he calls *Nematophycus Hicksii*. The resemblance generally between this very early (probably oldest known) flora, and the far more recent Devonian, is very marked, and indicates a tolerable uniformity in the character of the conditions, at those widely separated periods. The stratigraphical position of these plant-bearing beds may be stated to be about the horizon of the Llandovery rocks. They are shown in the Survey sections to rest immediately upon the Bala series, and some of the beds are even included in that group. The animal remains found in the same beds are all of marine forms, and the species would seem to indicate a gradual passage from Lower to Upper Silurian. The evidence seems to show that at this mid-Silurian period the immediate area where the plants are now discovered must have been under water, and that the mixture of marine and dry-land plants took place in consequence of floods or rapid marine denudation. The author indicated that the land-areas must have been to the south and west, chiefly islands, surrounded by a moderately deep sea, in which Graptolites occurred in abundance. (*Geol. Soc. Proc.*)

The Jurassic Rocks of England and France.—On the 27th April last the Rev. J. F. Blake communicated a most important paper to the Geological Society, in which he attempted to settle the many questions of correlation arising out of the detailed descriptions given of the various localities in the Paris basin where Upper Jurassic rocks are developed, by a consecutive survey of them all, undertaken by the aid of a grant from the 'Government Fund for Scientific Research.' In previous papers the names used for the great subdivisions and their boundaries were adopted without material modifications; in the present such modifications were proposed as may bring the English and Continental arrangements into harmony.

Five distinct areas were considered in this paper: 1. The Southern range; 2. The Charentes; 3. Normandy; 4. The Pays de Bray; 5. The Boulonnais.

1. *The Southern Range.*—This is continuous from the Ardennes through the Meuse, Yonne, &c., to the Cher. In the Ardennes the 'Ferruginous Oolite' corresponds to our Osmington Oolite, and to the Lower Limestones and Passage-beds of Yorkshire, the underlying 'Middle Oxfordian' being equivalent to our Lower Calcareous Grit. Above comes immediately the Coral Rag with *Cidaris florigemma;* and the stratigraphical and palæontological break is constantly between the Coral Rag and Ferruginous Oolite when that occurs. The Corallian is a well-marked formation, though its character is variability. It is divisible generally into two groups—Coral Rag and Supracoralline beds, the latter usually being the '*Diceras*-beds;' but in the Yonne there is a great development of '*Diceras*-beds below, associated with *Cidaris florigemma,* and massive corals, which are gradually introduced in going west. This part of the series in the Haute Marne has been described as very different; but the author did not at all agree with M. Tombeck's stratigraphical determination, and considers the 'Oolite de la Nothe' no more than the continuation of the Supracoralline *Diceras*-beds, which he considers to uniformly *overlie* and never to underlie the *Am. marantanus* marls, which latter are Oxfordian. In fact nothing abnormal occurs in this Department. The whole series has a tendency to degenerate into barren lithographic limestones, in which distinctions are lost. The Astartian and Virgulian beds were

traced through this range, the latter seldom showing any well-marked Pterocerian division, and the former being most connected with the overlying series. Above these are limestones hitherto called 'Portlandian,' in which two zones are constant; but above all are vacuolar Oolites, which alone may be truly correlated with the Portland rocks of England. The whole of the beds in this range are eminently calcareous, a true clay being scarcely anywhere seen.

2. *The Charentes.*—In these two Departments the lower portion is very calcareous, and the distinction of one part from another very slight; but the highest portion, both near Cognac and on the Ile d'Oleron, yields beds which may be paralleled with our true English Portland rocks.

3. *Normandy.*—The complete sequence has here been made out, from the true Oxford Clay of Dives to the Virgulian of Havre, and the similarity of the whole to the sequence in Dorsetshire is very remarkable. 'The Trouville Oolite' is the exact representative of the 'Osmington Oolite' with the Nothe Grits below; but the place of the Sandsfoot clay is taken by the true Coral Rag, whose right position in the Weymouth section is hereby determined. The Supracoralline beds are the sands of Glos, and the Astartian beds are the *Trigonia*-beds of Havre, which are the exact representatives of the 'Kimmeridge passage-beds.'

4. *The Pays de Bray.*—Nothing below the Virgulian is here seen, and the commencement of the so-called 'Portland beds' was considered by the author to be at a lower level than it is placed by M. de Lapparent, on account of the similarity to beds at Boulogne. The true Portland rocks occur as ferruginous sandstones with *Trigonia gibbosa.*

5. *Boulonnais.*—The Houllefort limestone was correlated with the Osmington Oolite. The Coral Rag of Brucdale was considered equivalent to that of the Mont des Boucards, the so-called limestones of the latter place being Supracoralline. The Nerinean Oolite and the Grès de Wirvigne represent the Astartian. The higher parts of the series have been already correlated.

From this study it was proposed—that the 'Lower Calcareous Grit,' and almost all the Coralline Oolite, should be placed in the Oxfordian series as the upper division, under the name 'Oxford Grit' and 'Oxford Oolite;' that the Corallian consists of two parts, the Coral Rag and the Supracoralline beds; that the Kimmeridgian should include the Astartian and Virgulian, the Pterocerian being a subzone; that the 'Upper Kimmeridge' and the Hartwell clay, with the 'Portland sand,' should make a new subdivision to be called Bolonian (the northern and southern types being both represented at Boulogne), which may be divided into Upper and Lower; that the true Portland limestone and the Purbeck be united into one group, as Lower and Upper Portlandian; the fact of the latter being freshwater being paralleled by parts of the true Portland having that character.

Changes of Level in Lancashire.—In a paper on the date of the last change of level in Lancashire, read before the Geological Society on the 6th April by Mr. T. Mellard Reade, the author described some observations made by him at Blundellsands, on the coast of Lancashire, near Liverpool, according to which, judging from the position of high-water mark, the land had gained considerably upon the sea between 1866 and 1874. At one end

of a length of 350 yards, spring-tide high-water mark had receded 15 yards, and at the other end 5 yards. The author estimated that the deposit of sand that had accumulated in eight years amounted to an average of 10 yards wide and 2 yards deep. Allowing a further depth of one yard for sand that may have been blown over the top, he finds 10,500 cubic yards as the quantity of sand deposited in eight years on a shore-frontage of 350 yards, or 3·75 cubic yards per yard of frontage per annum. Applying this unit of measurement to the 16 miles of coast forming the western boundary of the deposit, he gets 105,600 cubic yards as the quantity of sand annually moved; 22 square miles of sand, 12 feet thick, give 272,588,800 cubic yards of sand accumulated, which, divided by the annual quantity, will give 2580 years as the age of the whole deposit of blown sand. The author adduced other evidence in support of his view, and concluded that if the last change of level in south-west Lancashire was a downward one, it could not have taken place within 2500 years.

The Proteaceæ.—Count G. de Saporta has lately communicated to the French Academy an important note on the subject of the supposed presence of Australian forms of Proteaceæ in the European tertiaries. The Proteaceæ are at present limited to the Southern Hemisphere, and form two natural associations,—an Australian, including the genera *Petrophila, Grevillea, Lomatia, Banksia, Dryandra,* &c., which do not occur in Africa; and a South African represented by *Protea, Leucadendron, Leucospermum,* &c., which are not found in Australia. The plants which have been regarded as Proteaceæ in European deposits, from the late cretaceous of Aix-la-Chapelle upwards, have been referred to Australian genera such as *Grevillea, Lomatia, Banksia,* and *Dryandra.* Ad. Brongniart with some doubt indicated a *Stenocarpites* among the species brought from Koumi by Gaudry. M. de Saporta notices certain examples which render the identification of European fossil plants with the Australian types of Proteaceæ at least very doubtful. *Stenocarpites anisolobus,* Brongn., named by Saporta *Grevillea anisoloba,* has been recognized by Unger as representing the detached leaflets of a large Araliaceous leaf (*Cussonia polydrys,* Ung.). Nearly all the fossil leaves referred to *Grevillea* have a venation such as occurs in the Thymeleæ. Unger's *Dryandroides* and most of his *Banksites* are referred to the Myriceæ. *Dryandra Schrankii* (or *Comptonia dryandræfolia*) shows Myricaceous fructification. *Dryandra Contzeniana* and *primæva,* Deb. from the Senonian of Aix-la-Chapelle are probably *Comptoniæ. Dryandra Michelotii,* Wad. of the Paris Eocene and the Arkose of Brives, still, however, presents the characteristic physiognomy of *Dryandra;* but as M. de Saporta remarks, this shows how the evidence in favour of the Australian element in the European fossil flora has fallen off in value. No authentic fruit or seed has ever been found.

In the gypsum of Aix, in Provence, the *Lomatites,* especially *L. aquensis,* Sap., present a great resemblance to *Lomatia linearis* and *longifolia* of Australia, but this argument loses much of its force after comparison with the leaves of *Baccharis semiserrata,* De Cand. var. *glabra,* a Brazilian plant, of the order Compositæ. These leaves so closely resemble those of *Lomatites aquensis,* Sap., that it is hardly possible to resist the conclusion of a general affinity between the fossil and this living plant. Nevertheless there are

differences which would lead to the formation of a section or subgenus peculiar to the Tertiary of Provence. The texture of the leaves must have been more coriaceous; the marginal teeth are spinescent, and the veins generally concealed. These peculiarities and the general form caused the fossil plant to be referred to *Lomatia;* but M. de Saporta thinks it more natural to recognize the plant as belonging to the Baccharideæ and to name it *Baccharites aquensis.* This view is supported by the occurrence of numerous achenes or fruit of Compositæ in the same beds with the leaves. These organs, known as *Cypselites,* include at Aix at least four species, *Cypselites gypsorum, stenocarpus, Philiberti,* and *socius.* The last two agree entirely in characters with the achenes of *Coniza* and *Baccharis;* the body of the fruit is more or less spindle-shaped at the base, narrowed above, truncate at the apex, where it bears a sessile pappus of which the simple, silky hairs appear to be arranged in a single series.

If these facts be accepted as definitive and lead to the abandonment of the notion that we have Proteaceæ of Australian type represented in Europe, we should get rid of a great anomaly. The presence in Europe of types of plants now exotic is generally in relation to the present geographical distribution of those types. In this way many well-characterized genera, some now specially African, others limited to certain parts of Asia, or North America, have certainly formerly inhabited Europe. Successive revolutions with gradual cooling of the climate, will explain the disappearances that have taken place; but the direct implantation in the heart of ancient Europe of a whole colony of plants, such as are now quartered in one part of the Southern Hemisphere, with no outposts in the intermediate space, requires for its acceptance the most absolute proof. (*Comptes Rendus,* 16th May, 1881.)

Affinities of Halysites.—Prof. Verrill refers to the various genera of so called 'tabulated corals,' which have lately had their true zoological positions determined. The hydroid nature of *Millepora* is now generally admitted; he has himself shown that *Pocillopora* and its allies are Madreporarian corals, and that *Favosites* and related extinct genera are nearly related to the existing *Alveoporæ* and *Porites;* and Moseley, having ascertained the Alcyonarian affinities of *Heliopora,* has referred some fossil genera to the same position. With regard to *Halysites,* Prof. Verrill says, 'The affinities of the genus *Halysites,* the common "chain coral" of the Silurian, have hitherto been very doubtful. Within a few days Mr. H. T. Woodman has shown me a very remarkable specimen of this genus, in which the internal structure is beautifully preserved. In this example, which is a fragment several inches across, the large tubes contain twelve well-developed and regular septa extending to the centre. Their edges are slightly serrulate, and do not rise above the tubes. In other words, the structure is that of a true Madreporarian coral.' Mr. Woodman informed him that this specimen was a fragment from a larger mass eight or ten feet across, the greater part of which 'was like the common specimens, showing no rays; but here and there, in spots, all over the face of the mass, the septa were as well preserved as in the fragment.'

The Bears of the Cavern of Lherm.—M. Filhol has communicated to the French Academy (*Comptes Rendus,* April 11) some interesting particulars as

to the species of Bears found fossil in the bone-cave of Lherm, in the Ariège. Of the various mammals found in this cavern (*Ursus spelæus*, *Felis spelæus*, *Hyæna spelæa*, and species of *Rhinoceros*, *Cervus*, &c.), the Great Cave Bear (*Ursus spelæus*) is the most abundant, at least a hundred crania of that animal having been found. According to M. Filhol, these abundant materials prove the great fixity of character of the species, and whatever modifications some of them may show, they are always quite distinct from the common Brown Bear (*Ursus arctos*). Lately, M. Marty has found two skulls of Bears quite distinct from any that had previously been met with. One of them, a perfect skull, measures along its lower surface, from the incisor margin to the occipital foramen, 35 centim. (nearly 14 inches), and has six teeth behind the canine as in *Ursus arctos*, instead of only three as in *U. spelæus*. This piece of evidence is borne out by other characters; and M. Filhol identifies this animal with the living Brown Bear, which, therefore, he considers cannot have descended from the Great Cave Bear, but must have originated elsewhere, probably in North America, and gradually advanced to take the place of *U. spelæus* in Southern Europe, where it still occurs.

The other specimen is still more interesting. It consists of the anterior part of a bear's skull, having four teeth behind the canine in the upper jaw, and the first premolar separated from the canine by a space of 15 millim. Hence the face was very short, but it was remarkably wide. The width behind the carnassial tooth is 10·3 centim. The nasal aperture measures 64 millim. across, and only 51 from front to back, while in all other known bears the antero-posterior diameter is the longer. The forehead is depressed and nearly horizontally continuous with the nasal bones, its elevation above the palatine arch being only 10·8 centim. at a point where the forehead has an elevation of 11·8 centim. in the Brown Bear, and 18·3 centim. in the Great Cave Bear. The width of the forehead is nearly equal to that of the largest crania of *Ursus spelæus*. M. Filhol regards this fragment as indicating a new species of Bear, for which he proposes the name of *Ursus Gaudryi*.

A Jurassic Bird.—Prof. Marsh describes a fossil bird from the Jurassic *Atlantosaurus* beds of Wyoming, which will be the oldest representative of the class if *Archæopteryx* is to be regarded as Reptilian. Prof. Marsh names the species *Laopteryx priscus*. The type specimen consists of the posterior portion of the skull, which indicates a bird rather larger than a blue heron (*Ardea herodias*). The bones of the skull are pneumatic. The occipital condyle is sessile and hemispherical, flattened and slightly grooved above. The occipital foramen is nearly circular. The quadrate bone had an undivided head. In general character the skull seems most nearly to approach the Ratitæ. A single tooth was found in the matrix, resembling those of *Ichthyornis*. Prof. Marsh infers that *Laopteryx* possessed teeth and biconcave vertebræ.

MINERALOGY.

Plumosite in the Isle of Man.—In the *Transactions of the Royal Geological Society of Cornwall*, Mr. Warington W. Smyth notices the occurrence of plumosite in the Foxdale Mine, Isle of Man. The mineral is a double sulphide of lead and antimony, and its occurrence here is especially interesting as no antimonial minerals had previously been known from these mines, which furnish a richly argentiferous lead ore. Plumosite occurs in the Hartz, where it is known by the cognate name of *Federerz;* in Britain it is exceedingly rare. The specimens described by Mr. Warington Smyth consist, as usual, of very delicate fibres, which appear to be matted together like a tuft of dark cotton-wool. The microscope shows it to form prismatic crystals of brilliant metallic lustre.

Jadeite.—M. Damour has communicated to the Academy of Sciences (*Comptes Rendus*, 6th June, 1881) the results of some new investigations made by him upon jadeites and other sodiferous rocks. Jadeite is employed in India and China in the manufacture of vases and ornaments, and is also met with in the worked state in Europe, in prehistoric burial-mounds and caves.* The fact that no stone of the kind has ever been found in the rough state in Europe, has led to the assumption that the objects found here were imported by immigrants from the East, and M. Damour has investigated its geographical distribution with the view of clearing up the question of the real origin of the European jadeite axes and ornanents. M. Damour gives analyses of undoubted jadeites from Asia, and of portions of worked stone possessing the same characters obtained in Mexico, and in comparison with these some analyses of minerals obtained in Europe, especially in the region of the Alps, which he thinks make out a strong case in favour of the occurrence of jadeite in Europe. The following are the analyses of the European specimens:—

	I.	II.	III.	IV.	V.	VI.
Silica . .	58·51	56·45	57·99	55·82	56·74	54·53
Alumina . .	21·98	17·02	20·61	10·95	10·02	14·25
Ferric oxide .	1·10	7·62	2·84	5·68	4·69	3·29
Chromic oxide .	——	——	——	——	0·03	——
Lime . .	5·05	4·76	4·89	13·42	14·00	12·40
Magnesia . .	1·70	2·32	3·33	9·05	9·10	7·50
Soda . .	11·84	11·46	9·42	6·74	5·40	6·21
Potash . .	traces	traces	1·50	traces	traces	traces
	100·18	99·63	100·58	101·66	99·98	98·18
Spec. Grav.	3·35	3·17	3·16	3·22	3·32	3·31

No. I. is a specimen from Monte Viso, in Piedmont; II. from Ouchy, near Lausanne; III. is part of a stone axe; IV. is a rock from near St. Marcel, in Piedmont, where it was obtained from a small vein; V. from a pebble from

* See an article by F. W. Rudler in *Popular Science Review* for October 1879.

the valley of Aosta; and VI. a green substance containing garnets, forming a vein in gneiss near Nantes.

The author remarks upon the similarity of these minerals, both in character and composition, to the true jadeites, which are also generally found, not in position, but in the form of pebbles and blocks transported by glaciers or by running water. In Thibet there is no doubt that deposits of jadeite must exist, and the author infers that it also occurs in America, probably in Mexico and in the regions bordering on the Amazons. The analyses and observations given by him in his present paper would seem to show that the same mineral exists also on the European continent, probably in or near the chain of the Alps. If this be the case, there is no need to invoke a migration of ancient Asiatic populations to account for the presence of jadeite axes on this continent.

Geniculated Zircons.—Mr. W. E. Hidden describes and figures (*Silliman's Journal*, June 1881) twin crystals of zircon obtained by him from the new locality of apatite and sphene at Renfrew, Canada. The twinning plane is the same as in cassiterite and nitrite. The twin figured by him weighed over 35 grammes, and was remarkably perfect and had well-polished planes. Its colour was deep brown-lake, and it was transparent in spots. Previously described twins of zircon have been microscopic and questionable.

PHYSICS.

Electric Storage of Dynamical Energy.—The following article and letters, which have appeared in the *Times* during May and June, mark such an epoch in the history of electricity that they deserve reproducing *in extenso*:—

'We publish this morning a letter from Sir William Thomson, supplementary to that in which "F. I. R. S.," on the 16th of May, gave the first account of the conveyance from Paris to Glasgow of a box containing active electricity in a portable form; and it would certainly appear, from both communications, that applications of ever-widening utility are opening up before the most potent of natural forces, which seems to be rapidly becoming also the most manageable. Our readers will remember that "F.I.R.S." related how he witnessed in Paris, on the 9th of May, the charging of four of Faure's batteries with electricity obtained from an ordinary Grove's battery. The four batteries were enclosed in a wooden box, about a cubic foot in measurement, and weighing, with its contents, about 75 lbs. This box of "electric energy" was handed by M. Faure to our correspondent, who brought it with him across the Channel, and, after seventy-two hours of travelling, delivered it to Sir William Thomson at Glasgow, in order that the power of its charge might be made the subject of observation and experiment. The box was believed to contain electricity in quantity equivalent to a million foot pounds; and we now hear that this belief has been fully borne out by experiment. No appreciable loss was incurred during the transit; and the energy put into the four batteries in Paris remained in them until it was applied to working purposes in Glasgow. So completely was

the apparatus under command that one of the four batteries was detached from the rest, and carried to another place to supply the force required for an electric cautery. A single battery, when recharged, was left alone for ten days, and then it yielded to Sir William Thomson 260,000 foot pounds, so that the original estimate of a million foot pounds for the whole box was probably somewhat under the mark. Further investigations are required in order to discover what are the limits, if any, of the power to preserve electric energy unwasted; but sufficient is known to show that it can be preserved long enough for many important practical applications. That which Sir William Thomson regards as likely to be the first, at least in point of time, is the use of Faure's batteries in private houses, as reservoirs of electricity for domestic purposes, such as lighting, heating, working of sewing machines, so that no interruption of action would be produced by a temporary interruption of the electric supply given by any main engine from which it was derived.

'Sir William Thomson reminds us that the storage of force to be afterwards put to practical uses is a very old contrivance, of which we may see an illustration in the winding of a watch; and also the storage of electricity has long been familiar to men of science as a theoretical possibility. There is often a long step, however, from theoretical possibility to practical achievement; and what we are now called upon to notice is no longer that such a storage may perhaps be accomplished at some future time, but that it has actually been accomplished upon what may almost be called a working scale. Many philosophers are of opinion that electricity, at no distant date, will entirely supersede fires for cooking and heating, steam as a motor, and gas and oil as illuminants; but hitherto it has been felt as a great difficulty, economical rather than scientific, that the agents proposed to be superseded were themselves actually employed to produce the electricity. It has been asserted, moreover, that the sectional area of the metallic conductors required to deliver electricity in quantities suited to the requirements of town communities would have to be enormously large, and in a corresponding degree costly; and that difficulties with regard to insulation would be of constant occurrence. But electricity is to be obtained from the atmosphere by simple mechanical means wherever any kind of motive power is available; and, if it can be stored in portable batteries and carried about for use, there seems to be no reason why it should not be obtained in very cheap ways, as by wind or water mills, by mechanical contrivances for utilizing the ebb and flow of the tides, or by other applications of the power which nature everywhere offers for the use of man, and which is so frequently suffered to run waste.'

'Sir,—The marvellous "box of electricity" described in a letter to you, which was published in the *Times* of May 16, has been subjected to a variety of trials and measurements in my laboratory for now three weeks, and I think it may interest your readers to learn that the results show your correspondent to have been by no means too enthusiastic as to its great practical value. I am continuing my experiments to learn the behaviour of the Faure battery in varied circumstances, and to do what I can towards finding the best way of arranging it for the different kinds of service to which it is to be applied.

At the request of the Conseil d'Administration of the Société de la Lumière, I have gladly undertaken this work, because the subject is one in which I feel intensely interested, seeing in it a realization of the most ardently and unceasingly felt scientific aspiration of my life—an aspiration which I scarcely dared to expect or to hope to live to see realized.

'The problem of converting energy into a preservable and storable form, and of laying it up in store conveniently for allowing it to be used at any time when wanted, is one of the most interesting and important in the whole range of science. It is solved on a small scale in winding up a watch, in drawing a bow, in compressing air into the receiver of an air-gun or of a Whitehead torpedo, in winding up the weights of a clock or other machine driven by weights, and in pumping up water to a height by a windmill (or otherwise, as in Sir William Armstrong's hydraulic accumulator) for the purpose of using it afterwards to do work by a waterwheel or water pressure on a piston. It is solved on a large scale by the application of burning fuel to smelt zinc, to be afterwards used to give electric light or to drive an electro-magnetic engine by becoming, as it were, unsmelted in a voltaic battery. Ever since Joule, forty years ago, founded the thermodynamic theory of the voltaic battery and the electro-magnetic engine, the idea of applying the engine to work the battery backwards, and thus restore the chemical energy to the materials so that they may again act voltaically, and again and again, has been familiar in science. But with all ordinary forms of voltaic battery the realization of the idea to any purpose seemed hopelessly distant. By Planté's admirable discovery of the lead and peroxide of lead voltaic battery, alluded to by your correspondent, an important advance towards the desired object was made twenty years ago; and now by M. Faure's improvement practical fruition is attained.

'The "million of foot pounds" kept in the box during its seventy-two hours' journey from Paris to Glasgow was no exaggeration. One of the four cells, after being discharged, was recharged again by my own laboratory battery, and then left to itself absolutely undisturbed for ten days. After that it yielded to me 260,000 foot pounds (or a little more than a quarter of a million). This not only confirms M. Reynier's measurements, on the face of which your correspondent's statement was made; it seems further to show that the waste of the stored energy by time is not great, and that for days or weeks, at all events, it may not be of practical moment. This, however, is a question which can only be answered by careful observations and measurements carried on for a much longer time than I have hitherto had for investigating the Faure battery. I have already ascertained enough regarding its qualities to make it quite certain that it solves the problem of storing electric energy in a manner and on a scale useful for many important practical applications. It has already had in this country one interesting application, of the smallest in respect to dynamical energy used, but not of the smallest in respect to beneficence, of all that may be expected of it. A few days ago my colleague, Prof. George Buchanan, carried away from my laboratory one of the lead cells (weighing about 18 lbs.) in his carriage, and by it ignited the thick platinum wire of a galvanic *écraseur* and bloodlessly removed a nævoid tumour from the tongue of a young boy in about a minute of time. The operation would have occupied over ten minutes if performed by the

ordinary chain *écraseur*, as it must have been had the Faure cell not been available, because, in the circumstances, the surgical electrician, with his paraphernalia of voltaic battery to be set up beforehand, would not have been practically admissible.

'The largest useful application waiting just now for the Faure battery—and it is to be hoped that the very *minimum* of time will be allowed to pass till the battery is supplied for this application—is to do for the electric light what a water cistern in a house does for an inconstant water supply. A little battery of seven of the boxes described by your correspondent suffices to give incandescence in Swan or Edison lights to the extent of one hundred candles for six hours, without any perceptible diminution of brilliancy. Thus, instead of needing a gas-engine or steam-engine to be kept at work as long as the light is wanted, with the liability of the light failing at any moment through the slipping of a belt—an accident of too frequent occurrence—or any other breakdown or stoppage of the machinery, and instead of the wasteful inactivity during the hours of day or night when the light is not required, the engine may be kept going all day and stopped at night, or it may be kept going day and night, which will undoubtedly be the most economical plan when the electric light comes into general enough use. The Faure accumulator, always kept charged from the engine by the house-supply wire, with a proper automatic stop to check the supply when the accumulator is full, will be always ready at any hour of the day or night to give whatever light is required. Precisely the same advantages in respect of force will be gained by the accumulator when the electric town supply is, as it surely will be before many years pass, regularly used for turning lathes and other machinery in workshops and sewing machines in private houses.

'Another very important application of the accumulator is for the electric lighting of steamships. A dynamo-electric machine of very moderate magnitude and expense, driven by a belt from a drum on the main shaft, working through the twenty-four hours, will keep a Faure accumulator full, and thus, notwithstanding irregularities of the speed of the engine at sea or occasional stoppages, the supply of electricity will always be ready to feed Swan or Edison lamps in the engine-rooms and cabins, or arc lights for mast-head and red and green side lamps, with more certainity and regularity than have yet been achieved in the gas supply for any house on *terra firma*.

'I must apologize for trespassing so largely on your space. My apology is that the subject is exciting great interest among the public, and that even so slight an instalment of information and suggestions as I venture to offer in this letter may be acceptable to some of your readers.

'I remain, your obedient servant,

'WILLIAM THOMSON.

'*The University, Glasgow, June* 6.'

'SIR,—Although agreeing with every word of Sir William Thomson's letter in the *Times* of to-day, and entirely sympathizing with his enthusiasm as regards the marvellous box of electricity, still I feel that it would have been desirable if, in pointing out the importance of this new discovery, Sir William Thomson had guarded against a very probable misconstruction of the purport of his letter.

'The means of storing and re-storing mechanical energy form the aspiration not only of Sir William, but of every educated mechanic. It is, however, a question of degree—of the amount of energy stored as compared with the weight of the reservoir, the standard of comparison being coal and corn. Looked at in this way, one cannot but ask whether, if this form of storage is to be the realization of our aspirations, it is not completely disappointing. Large numbers are apt to create a wrong impression until we inquire what is the unit. Eleven million foot pounds of energy is what is stored in 1 lb. of ordinary coal. So that in this box, weighing 75 lbs. there was just as much energy as in $1\frac{1}{2}$ oz. of coal, which might have been brought from Paris, or anywhere else, in a waistcoat pocket, or have been sent by letter.

'When we come to the question of the actual conveyance of energy for mechanical purposes, this view is of fundamental importance. The weight of the same amount of energy in the new form is 800 times greater than the equivalent amount of coal; and as a matter of economy, supposing that energy in this form might be had at a certain spot, and no capital were required for its conversion or storage, and that the energy were directly applicable, it could not be carried ten miles, that is to say, such energy cannot be economically useful ten miles from its source, although coal had to be carried one hundred miles to the spot. This limit, in truth, falls far short of what has been already attained by other means. By wire ropes and by compressed air or steam energy may be economically transmitted from ten to twenty miles. So that if this is the utmost of what is to be done by means of the storage of electricity, this discovery adds another door to those which are hopelessly closed against the possibility of finding in Niagara or other water-power a substitute for our coal, even when the object is motive power, and much more for that purpose for which five-sixths of our coal is used—the production of heat.

'It is very important that the people of this country should not shut their eyes to the fact that so far from there being a greater prospect of the solution of the problem than when, about twenty years ago, Prof. Jevons raised the alarm, the prospect is now much smaller. In the meantime the capabilities of steel ropes, fluids in pipes, and electricity along conductors, have been not only investigated, but practically tested and found altogether wanting. And now, it would seem, that the storage of electricity must be added to the list.

'OSBORNE REYNOLDS.

'*Owens College, June* 9.'

'SIR,—Your leading article in the *Times* of yesterday on the storage of electricity alludes to my having spoken of Niagara as the natural and proper chief motor for the whole of the North American Continent. I value the allusion too much to let it pass without pointing out that the credit of originating the idea and teaching how it is to be practically realized by the electric transmission of energy is due to Mr. C. W. Siemens, who spoke first, I believe, on the subject, in his Presidential Address to the Iron and Steel Institute in March, 1877. I myself spoke on the subject in support of Mr. Siemens's views at the Institution of Civil Engineers a year later. In May, 1879, in answer to questions put to me by the Select Committee of the House of Commons on Electric Lighting, I gave an estimate of the quantity

of copper conductor that would be suitable for the economical transmission of power by electricity to any stated distance; and, taking Niagara as example, I pointed out that, under practically realizable conditions of intensity, a copper wire of half an inch diameter would suffice to take 26,250-horse power from water-wheels driven by the Fall, and (losing only twenty per cent on the way) to yield 21,000-horse power at a distance of 300 British statute miles; the prime cost of the copper amounting to 60,000*l.*, or less than 3*l.* per horse-power actually yielded at the distant station.

'I remain, Sir, your obedient servant,

'WILLIAM THOMSON.

'*The University, Glasgow, June* 9.'

'SIR,—Prof. Osborne Reynolds' letter in your issue of Saturday, the 11th inst., shows that the first idea that has occurred to him on reading Sir William Thomson's letter on Faure's "electric store," is probably what must have suggested itself to many engineers, viz., that so far from a million foot pounds being a surprisingly large amount of energy to be stored up in a mass of 75 lbs., it is really extremely small; and, indeed, while crossing over from Paris at the commencement of last week, I could not help thinking that the passengers were bringing to England literally in the smuts and blacks on their coats far more energy than had ever been imported into this country stored up in Faure's secondary batteries. But although it is perfectly true, as Prof. Reynolds says, that $1\frac{1}{2}$ oz. of coal contains about one million foot pounds of work stored up in it, this is by no means all that has to be taken into account in considering this question; for where is the engine for extracting this million foot pounds of work out of the $1\frac{1}{2}$ oz. of coal? Indeed, as Prof. Reynolds would himself tell us, we cannot get much more than one-tenth of this amount of work out of the $1\frac{1}{2}$ oz. of coal even in our largest steam-engines, which burn many pounds of coal per minute, and in which much heat is wasted in getting up steam. And if we come to the burning of one single $1\frac{1}{2}$ oz. of coal, I know of no engine that can obtain from this even one thousand foot pounds of work, or one thousandth of the energy contained in the coal, if no other coal be used in getting up steam or in previously heating the engine.

'But if a secondary battery be allowed to drive a magneto-electro-motor or a dynamo-electric machine with separate exciter only even for the short time necessary to develope, say, thirty foot pounds of mechanical work, I anticipate this can be done without using up in the whole process more than about thirty-five foot pounds of the electric energy stored up in the reservoir, since the experiments of Prof. Perry and myself have shown that, when the motor is running at high speeds with a light load, as much as ninety-three per cent of the electric energy put into a magneto-electro-motor is given out again as mechanical work measured by an absorption dynamometer.

'It may be answered, however, that if a small bit of coal, although containing a vast store of energy, is not of much practical use in producing work, in consequence of the absence, up to the present time, of a proper converter of the coal's energy into mechanical work, at any rate a small galvanic battery (a little Daniell's cell, for example) is not only a vast storehouse of power, but contains a store which we have the means of converting without

appreciable loss into electric light or mechanical work. How, then, is a Faure's box a better store of electric energy than a little Daniell's cell? This question has precisely the same answer as "Why is a pinch of dry gunpowder better than a pinch of wet?" Not because the dry gunpowder has more energy in it than the wet, but because the energy stored up in the dry gunpowder can be all, if we wish, used up quickly, and an explosion produced, whereas that in the wet can only be utilized bit by bit. So seven Faure's boxes will illuminate one hundred Swan lamps for six hours, while seven Daniell's cells, or, indeed, twice that number, although possessing a store of power millions of times as great as that in Faure's boxes, will not illuminate a single Swan light.

'But while fully recognizing the great advance made in the subject by Planté, and the recent improvements introduced by Faure, I do not wish to give the impression that the problem is by any means completely solved, since, if the attempt that Mr. Perry and myself, no doubt like many other electricians, are making to convert at a low temperature the energy in coal into electric energy, meets with even a fairly satisfactory solution, then a fragment of coal, or, it may be, a puff of gas rich in carbon or carbonic oxide, will be a practical store of energy of incomparably greater value than any secondary battery.

W. E. AYRTON.

'*The City and Guilds of London Technical College,*
'*Finsbury, June* 13.'

Globe Lightning.—M. Trécul records that on August 25, 1880, during a thunder-storm in the day-time, he saw a very brilliant, luminous body issue from a dark cloud. It was nearly white, having only a slight yellowish tinge, was distinctly circumscribed, slightly elongated in form, and had the appearance of being 35–40 centims. long and about 25 centims. wide. The two ends were somewhat conical. This body was visible only for a few moments, when it disappeared seemingly by entering again into the cloud, but before its disappearance, it threw off a small quantity of its substance, *which fell vertically like a heavy body*, leaving behind it a luminous train, at the edges of which were reddish sparks, or rather globules, for their light was not radiant. The upper part of the train became sinuous. The little falling body divided and soon afterwards became extinguished just before it passed down behind the houses. No sound was heard, although the cloud was not distant. (*Comptes Rendus*, 28th March, 1881.)

The Speed of Thought.—It is not unusual to hear the expressions, 'quick as thought' and 'quick as lightning,' used as if they were synonymous; but there is a vast difference, comparatively speaking, between them. The electric impulse is practically instantaneous over, say, a mile of wire; but, if we may trust the experiments of Helmholtz and others, the wave of thought requires about a minute to traverse a mile of nerve. An electric shock is felt simultaneously in every part of the body, but the sensations of touch and of pain occupy an appreciable time in making their impressions on the sensorium. The interval between the reception of an impression by the brain, and its perception by that organ, is, doubtless, inexpressibly short; but as we can only test the speed of thought by noting the time elapsing between the application of the cause of the thought and the

exhibition of some indication of its reception, we find that the time occupied can be measured. Thus Hirsch, by means of a suitable apparatus, found that a touch upon the face was recognized and responded to by a predetermined signal operated by hand in one-seventh of a second. There is no doubt some loss in the purely mechanical operation of making the signal; but when the different senses are tested in this manner, and a mean taken of all the experiments, we find not only that the act of thinking is not so rapid as was imagined, but that the speed varies with different senses. Thus the sense of touch was found to respond in one-seventh of a second, that of hearing required one-sixth of a second to respond, and when the eye was tested, one-fifth of a second was occupied in recognizing the signal. The distances travelled by the nervous impulses in each of these cases, was as nearly as possible the same, and it follows therefore that the recognition of them required more time in some cases than in others. Simple as it may seem, a number of operations must be performed by the brain in receiving and recording the reception of the impression. There is the transmission of the sensation to the brain, its recognition, and then the determining to make the signal, the transmission of the determination to the muscles, and the movement of those muscles. Hirsch showed, as explained above, that less time was required to recognize a touch than a sound, and that it took more time to see than to hear, but the question still remained as to what part of the time occupied was consumed in the act of recognition. Donders, by means of some very ingeniously constructed apparatus, solved the question. He found that the double act of recognizing a sound and giving the response, occupied seventy-five thousandths of a second, of which forty thousandths were occupied in simple recognition, leaving thirty-five thousandths for the act of volition. One twenty-fifth of a second was occupied in judging which was first of two irritants acting upon the same sense; but a slightly longer time was necessary to determine the priority of signals sent by different senses, as those of hearing and seeing. These results were obtained from a man of middle age, the young were slightly quicker; but the average of many experiments showed that the time required for a simple thought was never less than the fortieth of a second. From these experiments we learn that the mind cannot perform more than twenty-four hundred simple acts in a minute, and that the stories we have heard from persons rescued from drowning are simply exaggerations.

ZOOLOGY.

Paired Fins of Sharks and Rays.—Mr. F. M. Balfour lately read before the Zoological Society a paper on the development of the skeleton of the paired fins of Elasmobranchs, considered in relation to its bearings on the nature of the limbs of the Vertebrata. The object of the investigations recorded in this paper was explained by the author to be twofold,—viz., on the one hand to test how far the study of the development of the skeleton of the

fins supported the view which had previously been arrived at by the author to the effect that the paired fins were the specialized and highly developed remnants of a once continuous lateral fin on each side; and on the other to decide between the views of Gegenbaur and Huxley, and Thacker and Mivart, as to the primitive type of fin-skeleton. The author pointed out that the results of his researches were entirely favourable to the view that the paired fins were structures of the same nature as the unpaired; and that they gave a more general support to the views of Thacker and Mivart. They clearly showed that the pelvic fins retain a more primitive character than the pectoral. Conclusions were drawn somewhat adverse to the views recently put forward on the structure of the fin by Gegenbaur and Huxley, both of whom considered the primitive type of the fin to be most nearly retained in *Ceratodus*, and to consist of a central multisegmented axis with numerous rays on its two sides. It appeared, in fact, that the development of the skeleton demonstrates that a biserial type of fin like that of *Ceratodus* could not have been primitive, but that it must have been secondarily derived from a uniserial type, by the primitive bar along the base of the fin (the *basi-pterygium*) being rotated outwards, and a second set of rays being developed on its posterior border.

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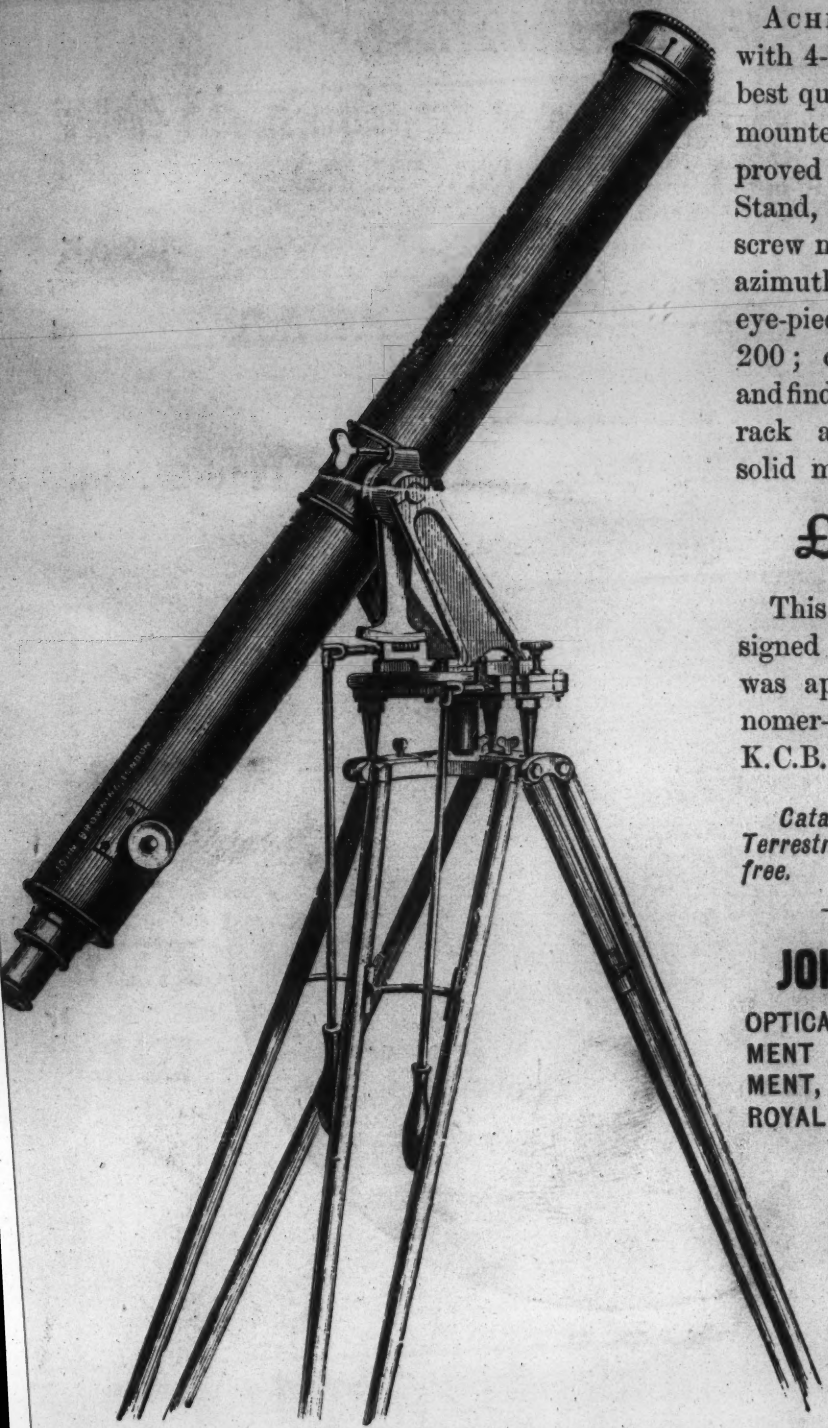

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